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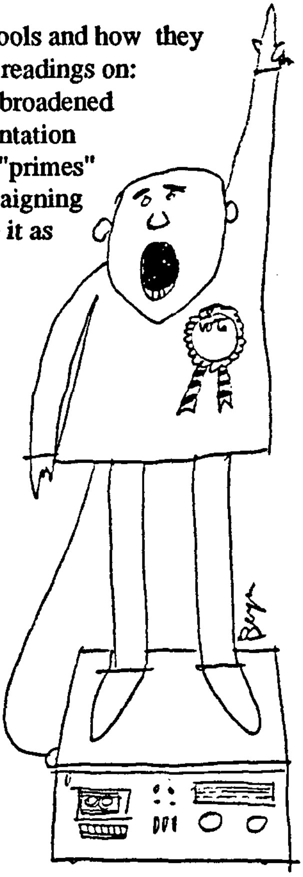

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ARTICLES

WHAT IS "RIGHT" IN HEGEL'S *PHILOSOPHY OF RIGHT*?

STEVEN B. SMITH
Yale University

I provide a thematic reconstruction of Hegel's positive concept of right. Against those who charge that Hegel denies any role to substantive political evaluation, I argue that the Philosophy of Right *articulates a notion of the right to recognition* (Anerkennung) *as the central feature of the modern state. The concept of recognition, I contend, requires not just toleration of others but a more robust notion of respect for the "free personality" that is the philosophical ground of right. The right to recognition is, furthermore, intended to provide the foundation for a new form of ethical life* (Sittlichkeit), *Hegel's modern analogue to classical conceptions of civic virtue. In conclusion I examine briefly two objections that stand in the way of a contemporary rehabilitation of Hegelian political philosophy.*

The concept of rights has recently undergone a revival in political philosophy. This might seem surprising given that the concept of human or natural rights has until recently been regarded as hopelessly passé, useful perhaps for Fourth of July speeches but outside the bounds of acceptable academic discourses. Indeed, the classic statement affirming the status of rights—"We hold these truths to be self-evident, that all men are created equal, that they are endowed by their Creator with certain unalienable Rights, that among these are Life, Liberty, and the Pursuit of Happiness"—is taken by many to be either meaningless or false. If *self-evident* means true by virtue of the terms involved, it is not difficult to show that by no means have these rights appeared to be self-evident to all (Hart 1979; Oppenheim 1957).

The Right of Recognition

Recently, though, the tide has begun to turn. We have been told to "take rights seriously" and that all human beings are endowed by virtue of their humanity alone to have a set of absolute and inviolable moral claims that take precedence over all competing reasons or policies. While rights claims, to be sure, are not scientifically demonstrable, they are thought to be morally necessary in the sense that without them we would have no grounds on which to attribute to the person an absolute and irreplaceable dignity. Furthermore, we would have no grounds for opposing policies that treat individuals as no more than an expression of impersonal social aggregates to be used in any way that serves the collective ends of society.

It is by no means obvious that a concern for the future of human rights should lead us back to a reconsideration of Hegel. Hegel is better known as a critic of rights than as a defender. In the first place, he attacked natural rights theories for proposing an "atomistic" conception of the self as denuded of all cultural traits and characteristics. Natural rights theorists from Hobbes to Kant (and more recently Rawls) typically claim to discover the most universal features of human beings by means of a kind of thought experiment, hypothetically stripping or peeling away everything we have acquired through the influence of custom, history, and tradition in order to discover the prepolitical state of nature and the natural man lurking behind it. In an early essay on *Natural Law* Hegel even criticized the "antisocialistic" theories of Kant and Fichte for denying the natural sociality of man and for "posit[ing] the being of the individual as the primary and supreme thing" (1975, 70; *Werke* 2:454).[1]

Second, he criticized rights theories as static, lacking any sense of the dynamics of human history and the developmental character of the moral personality. The self as Hegel understands it is not something "given" once and for all but is a being in the making, that is, a creature with a history. Whatever previous theorists might have claimed, rights claims are not static but are themselves part of a long and arduous historical process leading men gradually but inexorably toward an awareness of their own freedom. The idea that history represents a kind of collective *Bildung*—a moral education of the human race—toward a mutual recognition of right, I take to be Hegel's distinctive contribution to political philosophy.

This is not to say that Hegel thought it desirable to dispense with rights claims altogether. Rather he regarded rights as bound up with the dynamic structure of human history and especially the great revolutionary "moments" of the modern age—the Copernican, the French, and the Kantian. These events, he reasoned, were not isolated or discrete happenings but part of a worldwide struggle aimed at the realization of a certain desirable goal, namely, freedom (1956, 23; *Werke* 12: 38). If we look at history as previous historians have, namely by concentrating on particular events (the Peloponnesian War, the rise of Christianity, the reign of Louis XIV), we see nothing more than an interesting sequence of deeds with no connecting threads of rationality. But if we examine history as Hegel recommends, that is, not as a series of localized particulars but as a single process unfolding over time, we shall see in it the emergence of a "collective singular" (Koselleck 1985, 29).

The emergence of this conception of history as a collective singular made it possible to ask for the first time whether there was some point or meaning to history. Instead of regarding history in all of its infinite variety, Hegel conceived it as a struggle of different nations and cultures—Indians, Persians, Greeks, Romans, and modern Europeans—each trying to achieve freedom. Accordingly he believed it possible to divide history into a number of different epochs or stages, each based on the degree of freedom that had been achieved (1956, 18–19; *Werke* 12:31–32). Thus in the oriental world, Hegel could write, only one man—the despot—was free. In the Greco-Roman world some were free, the freeborn citizens of the various *poleis*. But in the modern world, disciplined by such events as the Protestant Reformation and the French Revolution, freedom is extended to all. Hegelian history, as W. H. Walsh has noted, represents nothing so much as the success story of modern European man (Walsh 1971, 183).

What made Hegel's argument about freedom anathema, especially to liberals, was his tendency to argue that it had been more or less realized in the modern European state. The famous prefatory remark in the *Philosophy of Right* declaring that "what is rational is real and what is real is rational" appeared to many as a blanket justification of the status quo however it stood.[2] Thus hostile critics from Rudolph Haym (1962) to Karl Popper (1963) have argued that for Hegel freedom has been fully and adequately realized in the Prussian state of the 1820s. But this is not what Hegel says at all. Hegel's *Philosophy of*

Right is not a justification for the Prussian monarchy but is rather the profoundest piece of philosophical jurisprudence in the modern world.

The state that Hegel has in mind is not identified with any particular or existing state but with the idea of the *Rechtsstaat,* a term for which there is no precise English equivalent but that is perhaps best captured in our phrase "the rule of law" (Oakeshott 1975, 257–63). Only in a state governed by law is freedom possible. By *a state governed by law* Hegel means one that extends the right of recognition (*Anerkennung*) or respect to every one of its members. It means the right to what members of the liberal tradition have taken to calling "equal concern and respect." Without some token of esteem or respect from one's neighbors, Hegel argues, none of the other goods afforded by society will have value. The various "categories" that structure social life, chiefly including civil society and the state, are not just conservative restraints on freedom but the necessary context for persons who mutually seek to acknowledge and enhance one another's right to recognition.[3]

It may be objected that the concept of recognition, while central to Hegel's *Phenomenology of Mind,* is downplayed, perhaps dropped altogether, in the later *Philosophy of Right.* As sober an interpreter as George A. Kelly has remarked that to see the struggle for recognition as a "regulative idea" guiding all of Hegel's thought is to risk "anachronistic overtones of the Marxian class struggle" (Kelly 1978, 31–32). But this is perhaps an overstatement. Hegel's argument is based upon the assumption that human agents are driven by a powerful common interest in rational freedom that is in turn logically tied to the concept of mutual recognition. The word *recognition* need not be literally present for the concept to function. Freedom is, for Hegel, an interactive concept. Human beings are free only when they see themselves expressed in their relations to nature and their social institutions. These two aspects of freedom are not unrelated. The first involves an awareness that we are both separate from and sovereign over nature, which includes not just the external world but our bodily desires and inclinations. The second presupposes the mutual recognition of each person within a framework supplied by law. The right of recognition, I will argue, is intended to provide the basis for a new form of ethical life (*Sittlichkeit*) for the modern world.

The Idea of Right

The subject matter of the *Philosophy of Right* is stated quite simply in the introduction to the text as "the idea of right" (1972, 14; *Werke* 7:29). As the term suggests, Hegel was not simply interested in the historical question of how the right order is brought into being but with such traditional questions as the right or just ordering of political relationships. The book would seem to be intended as an analogue not only to modern works like Hobbes's *Leviathan* and Rousseau's *Social Contract* but also to ancient studies like Plato's *Republic* and Aristotle's *Politics.* Yet the appearance of continuity with the past is at least partially deceptive. The term *right* in the title is ambiguous. The German *Recht* can mean either "right" or "law," and the phrase *philosophy of right* has a peculiar ring to it that *philosophy of law* does not. In its widest sense *Recht* refers to the entire normative structure of a people's way of life, not just their civil rights and liberties but the whole system of ethical norms and values—not to mention religious rules and precepts—informing a culture.

Hegel tends to distinguish *Recht* from *Gesetz,* the term he uses for law in the narrow sense when referring to civil or positive legal codes. Indeed, he makes much of the etymological point that the

German word for law, *Gesetz*, is related to the word for posit, *setzen* (1972, 134; *Werke* 7:361; see also Foster 1935, 119). It is preferable, therefore, to continue thinking of his book as philosophical inquiry into *Recht*, where *Recht* roughly means the entire range of practical reason. This is no longer Kant's *reine praktische Vernunft* but a matter of immanent rules proceeding from the rational will embedded in historical circumstances. Hegel himself gives credence to this interpretation when he says, "In speaking of Right ←*Recht*→ . . . we mean not merely what is generally meant by civil law, but also morality, ethical life, and world-history" (1972, 233; *Werke* 7:90–91).

Still, Hegel's meaning is not so much clarified as complicated by a glance at the subtitle of the work, namely, *Natural Right and the Science of the State in Outline* (*Naturrecht und Staatswissenschaft im Grumdrisse*). The term *natural right* is a traditional one that Hegel deems "not altogether correct." In one sense it points backwards to the normative theory of right that has its origins in classical antiquity. The ancients used this term to indicate what is by nature right or just in opposition to the rules or laws laid down by particular communities, which have their basis in arbitrary whim or fiat. On Hegel's view, however, right is crucially misunderstood if it is regarded as an expression of nature. Neither the external physical environment nor the internal sphere of human needs, wants, and desires can serve as an adequate basis for right. The term *natural right* is misleading whether it is understood to mean "something implanted by immediate nature [*unmittelbarer Naturweise*] or something determined by the nature of the thing [*Natur der Sache*]." Right, properly speaking, has its basis in the "free personality alone—on self-determination or autonomy, which is the very contrary of determination by nature." The term *natural right* should consequently be abandoned and replaced by the expression *the philosophical doctrine of right* (*die philosophische Rechtslehre*) (1971a, 248; *Werke* 10: 311–12).

This leads me, then, to my first thesis about right, namely, that it has its ground (*Boden*) in the individual subject or *Wille* (1972, 20; *Werke* 7:46). Hegel's starting point here is the minimal, or "thin," theory of the subject as a will capable of distinguishing itself from the rest of nature. Unlike ancient and medieval writers who sought to infer the proper ordering of human relations from our place within the whole, Hegel follows the lead of Hobbes, Rousseau, and Kant in denying that there are natural ends or purposes there to be discovered. There is no graduated scale of nature where there is a place for everything and everything has its place. Nature in fact provides no clues or evidence for how the moral order should be constructed. It is this fact that ultimately renders the term *natural right* so equivocal, since "nature is not free and therefore is neither just nor unjust" (1972, 44; *Werke* 7:113).

The *Philosophy of Right* takes the form of a phenomenology of the moral will. The will is simply the way the mind functions when it functions practically as opposed to theoretically. The first moment, or "determination" (*bestimmung*) of the will is defined by an abstraction from all content, from everything empirical or merely "given." What is left is the purely "negative will," the pure "I," which is characterized by a capacity for freedom. Hegel tries to explicate the freedom, or self-determination, of the will by an analogy to the sciences of nature:

> Freedom . . . is just as fundamental a character of the will as weight is of bodies. If we say: matter is "heavy" we might mean that this predicate is only contingent; but it is nothing of the kind, for nothing in matter is without weight. Matter is rather weight itself. Heaviness constitutes the body and is the body. The same is the case with freedom and the will, since the free entity is the

> will. Will without freedom is an empty word, while freedom is actual only as will, the subject. (1972, 226; *Werke* 7:46)

One might infer from this passage that the concept of freedom would result in a kind of nihilism, the condition in which everything goes. Hegel even implies as much when he remarks that "only in destroying something does this negative will possess the feeling of itself as existent." He refers, further, to "the fanaticism of the Hindu pure contemplation" and the "universal equality" pursued by the French revolutionaries as evidence of the nihilistoc goals of this purely negative freedom. It is the freedom identified with arbitrary choice (*Wilkür*) rather than with freedom under law. Thus while negative freedom has produced the "maximum of frightfulness and terror" and as such is a source of contemporary irrationalism, Hegel also maintains that the will has the resources to provide out of itself a new purified order of right and justice (1972, 22, 227–28; *Werke* 7:49–52).

The need for some kind of self-limitation leads to my second thesis about the will's activity. The need for limits is not a contradiction of freedom but essential to it. Freedom, as we shall see in the next section, does not imply a world ungoverned by law but one inhabited by subjects capable of supplying these principles themselves. Willing is not an arbitrary activity but already implies some minimal notion of a meaningful way of life within which willing and choosing can take place. Hegel's point is that willing presupposes a community of wills or rational agents whom we cannot choose to be without. Willing is never an isolated activity but always takes place within the context of a plurality of wills. It is the irreducible plurality of the human condition that makes willing a transaction between subjects, between an "I" and a "we" or, as he put it in the *Phenemenology of Mind*, between an "I that is a we . . . and a we that is an I" (1966, 227; *Werke* 3:145).

The subject of rights is, then, the "rational will" (*vernünftige Wille*), which Hegel characterizes as "self-determining universality" (1972, 25; *Werke* 7:62). So long as we understand the will to mean sheer "arbitrariness," it is not really free. For reasons similar to those of Rousseau and Kant, Hegel believed that such a view of freedom generally meant no more than slavery to natural appetites and desires. The self-determination of the arbitrary will is a "moment" of freedom but not yet developed, rational liberty. The moral will is characterized not just by a capacity for free choice but by deliberation and reflection on ends. While the arbitrary will may be able to pursue various impulses and desires, it has not yet attained control over its impulses and desires. The will understood as mere negative or arbitrary freedom can never be more than the Hobbesian "last appetite in deliberation" (Hobbes 1962, 54). Moral freedom contains, then, the capacity not just to desire but also to reflect evaluatively upon the kinds of things we ought to desire. It contains, in the last instance, the capacity to select and evaluate desires.

Hegel must answer the question of what the particular content of the will is or to what it can attach itself. The idea that right has its ground in the will appears to ignore the social basis of personality—that we are socially constituted in a variety of complex ways. His answer to this question is that in concentrating on the first aspect of the will—its ability to distinguish itself from all content—Hegel's early modern predecessors forgot that willing is also a teleological or purposive activity. To will is not merely to declare one's independence, it is to will something. Hegel here appears to return to an older position that was given its classic formulation by Aristotle, namely, that every human deliberate action is performed for the sake of some end to be brought about in the future. It is in "the nature of mind" (*die Natur des Geistes*)—

a phrase with obvious Aristotelian overtones—to express itself in specific institutions and activities (Taylor 1979, 76, n. 2). The will is not something prior to its action, or—to put it differently—a person cannot be totally detached from the kinds of commitments and choices he or she has made. Rather, the will is always embedded in an "objective" world of political and legal institutions that reach their fruition in the idea of the state (1972, 242; *Werke* 7:159).

This leads us to a third thesis about right, that is, that the will achieves its realization, or "substantive end," only in the state. This is the phrase that has so alarmed many of Hegel's critics. Now, to say that the will becomes rational and free only in the state is, to be sure, hard doctrine. Hegel's *etatisme* allegedly identifies freedom with obedience to the police. But what Hegel means by the state need carry none of these sinister implications. The Hegelian state is above all an organization of laws, a *Rechtsstaat.* Law is what purges the state of caprice and makes possible such modern freedoms as contract, property, career choice, religion, and speech. The result is by no means some kind of irrational state worship but the deepening of a recognition and respect for the wishes and ways of life of others, a manner of behavior that could be called "civility" (Oakeshott 1975, 108-84).

The core of the modern state is, then, respect for the person, or "free personality," as such. This is much different, for example, from the ancient world, where, according to Hegel's investigations, the individual had not yet learned to distinguish him- or herself from the environment but lived in an "immediate" condition of trust or faith with his or her surroundings. As Hegel's interpretation of Sophocles' *Antigone* indicates, the Greeks simply did not think of themselves as individual subjects capable of choice and deliberation but as accidents of certain all-powerful substances that had already sealed their fates in advance.[4] It is the exercise of the will—of free critical intelligence—and the desire to be *in* everything we do that most clearly distinguishes modern from ancient freedom. "This 'I will,'" he says, "constitutes the great difference between the ancient world and the modern, and in the great edifice of the state it must therefore have its appropriate objective and existence" (1972, 288; *Werke* 7:449).

The difference, then, between the ancient polis and the modern state is that far from recognizing the individual autonomy of each of its members, the polis was the paradigm of a tutelary community based on a shared moral understanding and directed toward a specific way of life. This conception of a closed homogeneous society was given its profoundest expression in Plato's *Republic,* which Hegel sees as "nothing but an interpretation of the nature of Greek ethical life." Unlike Socrates, whom Hegel interprets as a moral skeptic questioning all traditional values and institutions, Plato sought to close the lid on the Pandora's box opened by his teacher by requiring restraints on marriage, the family, and property. While Hegel commends Plato's "genius" for recognizing that "there was breaking into that life in his own time a deeper principle which could appear in it . . . only as something corruptive," his proposals in the *Republic* "did fatal injury to the deeper impulse which underlay it, namely, free infinite personality" (1972, 10; *Werke* 7:24).

The oppressive character of Plato's *Republic* is typically contrasted by Hegel to the principle of the will, or "infinite personality," that is recognized by the modern state. The person largely responsible for this principle is Rousseau, who in *The Philosophy of Right* is congratulated for "adducing the will as the principle of the state" and "not a principle like gregarious instinct . . . or divine authority" (1972, 156–57; *Werke* 7:400). The reference to Rousseau here is by no means accidental. Hegel frequently singles out Plato and Rousseau as the two thinkers most characteristic of ancient substantialism and modern subjectivity, respectively. What distinguishes modernity is precisely the emphasis on the will and individual consent as the core of right. In Plato's *Republic* "the subjective end simply coincided with the state's will. In modern times . . . we make claims for private conscience" (1972, 280; *Werke* 7: 410). And he later remarks that, "In Plato's state, subjective freedom does not count, because people have their occupations assigned to them by the guardians. . . . But subjective freedom, which must be respected, demands that individuals should have free choice in this matter" (1972, 280; *Werke* 7:410).

The Struggle for Right

It is well known that Hegel rejected the state of nature and social contract methodologies of his early modern predecessors. Their "abstract" individualism and lack of attention to the dynamic, developmental aspects of history are their most frequently cited deficiencies. What is less often noted, however, is that Hegel himself used a crypto-state-of-nature teaching to derive his theory of right. The account of "the idea of right" in the *Philosophy* of *Right* presupposes the famous "struggle for recognition" in the opening pages of the chapter on "self-consciousness" in the *Phenomenology* and his later clarification of this theme in the *Encyclopedia* version of the *Philosophy of Mind*. Every bit as much as Hobbes or any other contractarian, Hegel explains the origins of right by reference to a putatively "natural" condition that is one of maximum conflict and insecurity. Political life is not natural to men but required to rectify the inadequacies of nature.

Hegel presents the origin of right as laying in the desire (*Begierde*) of two individuals seeking some sign of recognition from one another. Hegel infers the desire for recognition from the very nature of self-consciousness. The mind that desires to know everything desires first of all to know itself. But how is self-knowledge acquired? Hegel's answer is that we come to know ourselves not by isolated introspection in the manner of a Descartes but through interaction with others. The mind is led to reflect back upon itself only after experiencing those around us. "Self-consciousness," he writes, "exists in and for itself, in that, and by the fact that it exists for another self-consciousness; that is to say, it is only by being recognized (*Anerkanntes*)" (1966, 229; *Werke* 3:145). The view of the self developed here could be called relational insofar as it sees us as parts of complex systems of mutual interaction that determine our identities.

The desire for recognition is, for Hegel, the quintessentially human desire. Hegel presents the will as containing a number of conflicting, even contradictory, desires, for instance, the desires for food, clothing, and shelter, each one of which cries out for satisfaction. But if we acted only to satisfy our biological urges, human existence would never rise above the state of nature. Obviously, the satisfaction of basic animal needs for warmth, food, and protection is a necessary but not a sufficient condition for the fulfillment of our truly human needs. Like Rousseau in the *Second Discourse*, Hegel is impressed by the elasticity of our desires. We are instinctually underdetermined (Rousseau 1964, 114). While the desire for food may be universal, there is a great deal of room left to determine how we should eat, when, where, and with whom. Furthermore, there is virtually nothing that cannot become an object of our desires. To use a vocabulary that is not Hegel's own but that, I hope, does not do violence to his meaning, it is because humans have the capacity to desire not only natural objects but also nonnatural objects or values that they are able to rise above the level of the brutes and become human at all. Our desires are not the product of sheer unmediated instinct but of will and reflection; they are *intentional* desires precisely as elaborated by H. P. Grice (1957, 377–88).

Hegel's concern could be put in the following terms. We begin with some object of immediate desire. Such an object is here conceived as a means to the fulfillment of some specifiable end. It is the kind of desire attributed to all of us all of the time by Hobbes when he wrote that "felicity is a continual progress of the desire, from one object to another, the attaining of the former being still but the way to the latter" (Hobbes 1962, 80).

But unlike Hobbes and later utilitarian writers, for Hegel this is only to state a problem, not to provide a solution. Human beings not only have desires of various kinds; we also have desires to have desires. Our identities are not fixed in stone; we can desire to have identities of a particular sort. We have the capacity, cognitively speaking, to stand back from our desires and ask whether they are the kinds of desires we wish to have. It is this desire to desire, or what Harry Frankfurt has called "second-order desires" (1971, 5–20), that leads us out of the infinite regress implied by Hobbes, where every desire is simply a means to another desire.

The desire for recognition is a desire unlike any other desire. It is not just a means to some specifiable end but a means to the enjoyment of any end whatever. If this sounds odd it is because we have been conditioned to think of desires as a part of our makeup opposed to rationality. But Hegel rejects this modern mind-body dualism in favor of a more complex relationship. Desires, he believes, entail rationality, and rationality involves desire. Our appetites are, so to speak, "shot through" with reason. Reason is not something superimposed on the passions from outside but is more like a principle of organization that works both in and through the passions (1971a, 235–36; *Werke* 10:296–97). Thus the desire to be recognized is not just another desire that we happen to have; it is the core human desire central to our sense of well-being, of who and what we are. We are beings who are not just constituted by a desire for comfort, safety, and security but who cannot live—or at least cannot live well—if our desires are not respected by those around us. What we desire, above all, is to be treated with a sense of decency and respect. Such treatment is necessary for our basic sense of self-respect.

Hegel's account of the struggle for recognition and the so-called "master-and-slave" relation that grows out of it is too well known to require much exegesis.[5] His main point is that the recognition to which each person believes him- or herself entitled is not immediately forthcoming. Each wants to be recognized without in turn having to grant recognition to others, and this one-sided and unequal state of affairs leads one to enter a life-and-death struggle not unlike the Hobbesian *bellum omnium contra omnes.* It is from this life-and-death struggle, in which humanity's passion for honor and prestige is asserted over its fear and terror at the possibility of violent death, that the all-important relationship between master and slave arises. This arises because in the struggle one of the parties is unwilling to go all the way and risk life for the sake of recognition, thereby submitting to the other, granting recognition without requiring it in turn. In short, the vanquished party subordinates its own desire for esteem to the biologically stronger desire for self-preservation.

Hegel's account of the struggle for recognition seems almost like a satire on Aristotle's account of slavery in book 1 of the *Politics.* For Aristotle slavery was justified because it was the political institution that corresponded most closely to the natural hierarchy, or inequality, between the body and the soul. Just as it is the function of the body to submit to the rule or governance of the soul, so is it the function of the slave to free from a life of drudgery and toil those few capable of engaging in political activity and philosophy. If nature provides a model, or paradigm, for our institutions, slavery has its origins in human nature itself. Aristotle, of course, uses his doctrine of natural slavery to show that not all existing slaves are in fact slaves by nature, as many have been taken as prisoners of war. Yet he elsewhere argues that just as the soul and body can work together to produce a well-functioning, or healthy, individual, so too is there a kind of common interest and even friendship possible between a master and a slave.

Hegel turns Aristotle on his head. The conceptual basis for slavery is the need of one self-conscious mind to be recognized by another. In the ensuing struggle the vanquished grants recognition to the lord by the very fact of being forced to work in the latter's service. The master's enjoyment is predicated upon freedom from work. However, the recognition that the master now enjoys is not that from an equal but from a degraded tool who is merely employed to satisfy the master's material comforts. The master ends up in the same position as Aristotle's "great-souled man" who desires honor and recognition but finds it unworthy once it has been bestowed (*Nicomachean Ethics* 1123b-25a [1975]). The master is somehow greater than any sign of recognition received. Rather than having gained a level of contemplative autonomy and self-sufficiency, the master comes to realize a dependence on the slave to satisfy desires, and this realization serves to undermine the asymmetry of the relationship.

Marxist interpreters have made much of Hegel's account of the origins of society, especially the role of slave labor in the production of culture. They point to the relatively greater importance Hegel assigns to making or fabricating—what the Greeks called *poiesis*—than on acting, or doing, (*praxis*) (Riedel 1969, 29–33). Whatever his later strictures against Hegel's idealism, Marx's own historical materialism was crucially dependent on Hegel as he himself recognized in the *1844 Manuscripts*. "Hegel's standpoint," he says there, "is that of modern political economy. He conceives labor as the essence . . . of man" (Marx 1978, 112).

Nevertheless, this interpretation can be overdone. Unlike his Marxist interpreters, Hegel views labor fundamentally as an intentional activity (Bernstein 1984, 14–39). It is an expression of the will, or free personality, and cannot be reduced to more rudimentary "material" determinants like external pressures or bodily needs. For the Marxist, intentionality is always secondary to material conditions, while for Hegel it is the essence of the human. Thus Hegel explains mastery and slavery as the outcome of a struggle not for self-preservation (a material end) but for recognition (a spiritual one).

Hegel's resolution to the conflict of master and slave seems unduly forced. Nevertheless, it provides a convenient transition from the struggle for recognition to the ethical sphere of "universal self-consciousness." Hegel defines this sphere as "the affirmative awareness of self in an other self," which is "the form of consciousness which lies at the root of all true mental or spiritual life—in family, fatherland, state, and of all virtues, love, friendship, valor, honor, and fame" (1971a, 176; *Werke* 10:226). From the context it is clear that what Hegel calls "universal self-consciousness" is a close approximation of his conception of ethical life, or *Sittlichkeit*. This means something more than the Kantian self-determination of the will. It is something like a common culture consisting of a set of shared ideas, norms, and values. The practices and institutions of ethical life—

family life, economic activity, and politics—are not just limitations on the will's activity but the social context within which freedom is possible. Only from within the concrete forms of ethical life is mutual recognition possible.

Recognition and Moral Personality

The point of the foregoing discussion was to show that Hegel's idea of right is not just tautologically posited to make sense of the modern state but historically constructed through a process of labor and struggle. Unlike a contemporary legal philosopher—say, Ronald Dworkin—who lays down a right to equal concern and respect and then goes about describing the kinds of social and political institutions necessary to sustain that right, Hegel regards the concept of right as tied to a distinctive conception of human personality (*Persönlichkeit*). "Personality," he writes, "essentially involves the capacity for rights" (1972, 37; *Werke* 7:95). Being a person means here having a sense of one's self as an autonomous agent with a will and consciousness of one's own.

The idea of right is only possible, then, where there is some universal conception of the self or personhood that is the designated bearer of right. What Hegel calls a "person" is essentially a legal entity entitled to disposition over the objects that have become its property. While property is defined simply as that over which we have acquired legal title, it follows that from the legal point of view it is "a matter of indifference" how much, if any, property a person possesses (1972, 44; *Werke* 7:112-13). All that matters is the individual's abstract capacity to acquire, utilize, and exchange property with other persons. Accordingly, the maxim regulating the behavior of such legal *personnae* is simply, "Be a person and respect others as persons" (1972, 37; *Werke* 7:95). This maxim is by no means idiosyncratic or capricious but is central to much of our legal reasoning. For whenever we think of persons as the law enjoins, we do so not on the basis of their specific accomplishments or character traits but as formally identical entities related only by their capacities to recognize and understand the law.

Hegel traces the legal concept of personhood back to the Roman Empire, when the essentially modern idea of "legal status" came to take precedence over active citizenship. Unlike the modern legal person, who claims rights against the state, the ancient citizen was regarded as a part of a larger ethical whole or totality. This conception of citizenship was given its canonical expression in book 1 of Aristotle's *Politics*, where it is expressly stated that the city is prior to the individual and that a human being is "by nature" a "political animal" capable of realizing faculties only through political participation. Consequently, "a man who is without a city [*apolis*] through nature rather than chance is either a mean sort or a being superior to man" like "the 'clanless, lawless, heartless' man reviled by Homer" (*Politics* 1253a [1977]). Aristotle actually calls the city a *koinonia politike*, a political association or community, to grasp better the nature of the civic tie. A community is a society not just of strangers but of friends or comrades (*heteroi*) whose lives are centered on certain common, corporate goals. The city is, in short, something literally "held in common" (Mulgan 1977, 13-17; Riedel 1969, 140-44).

All of this is quite different from the modern *Rechtsstaat*. Hegel's conception of the emergence of legal status is noteworthy especially because of its place within the various nineteenth- and twentieth-century theories of political modernization and development. Like Henry Maine, whose classic work, *The Ancient Law*, saw the development of the modern state in terms of a shift from status to contract, or Ferdinand Tönnies who characterized the same process as a movement from *Gemeinschaft* to *Gesellschaft*, Hegel sought to account for this as a move from the classical citizen to the modern bourgeois or *Bürger* (1972, 124, 127; *Werke* 7:343, 348). Unlike the citizen whose identity stemmed from membership in a particular community, the *Bürger* is defined precisely by freedom from all such parochial attachments and traditions. While a citizen is related to fellows by a shared moral understanding, the *Bürger* is a private individual who engages in competitive struggle with others in the arena of civil society. Thus, underlying the *Bürger*'s way of life is a formal equality expressed in a demand for mutual respect. For the *Bürger* "A man counts as a man in virtue of his manhood alone, not because he is a Jew, Catholic, Protestant, German, Italian, and so on" (1972, 134; *Werke* 7:360).

The right to recognition, we might say, is not simply a contingent feature of the modern state; it is its inner soul and purpose. What Hegel calls the right to recognition is not unlike what the liberal tradition has deemed as equal treatment before the law or what has recently come to be called the doctrine of equal concern and respect. Hegel says as much in a passage from the *Encyclopedia*: In the state "man is recognized and treated as a rational being, as free, as a person; and the individual, on his side, makes himself worthy of this recognition by overcoming the natural state of his self-consciousness and obeying a universal, the will that is in essence and actuality will, the law; he behaves, therefore, towards others in a manner that is universally valid, recognizing them—as he wishes others to recognize him—as free, as persons" (1971a, 172–73; *Werke* 10:221–22).

Hegel's defense of the right of recognition could take one of two strategies. The first, adopted variously by Kant, Rawls, and Dworkin argues that human beings are entitled to equal recognition not because of their substantive achievements but because of an underlying skepticism about the human good. Because, it is argued, opinions about the good are ultimately a question of value and thus incorrigible, the most appropriate political response is the construction of a constitutional framework that is neutral to substantive questions about the good. In the language of contemporary Kantian liberalism the right must take precedence over the good. Since there is no single or comprehensive goal for which we all strive, the optimum solution to the plurality of ends is something like the modern liberal state, which professes official indifference or neutrality toward the ways of life of its citizens.

This line of defense fails for two reasons. First, consistent skepticism about the good engenders not respect for persons but the opposite. Rather consistent skepticism of the type advocated by Max Weber promotes an unconstrained struggle between competing values and ways of life. "It is really a question not only of alternatives," Weber wrote, "but of an irreconcilable death struggle like that between 'God' and the 'Devil' " (Weber 1949, 17). Only if the parties in question have made a prior commitment not to be skeptical about equal respect will this defense not degenerate into a war of all against all.

The second flaw with skepticism is that on closer inspection it is frequently not skeptical at all. The value that the skeptic frequently elevates above all others is individual liberty. For the skeptic, the greatest political sin is governmental paternalism—the attempt to "legislate morality." Paternalism is ruled out because it violates our sacred right to choose for ourselves how to live. Perhaps the boldest defense of individual autonomy against the claims of governmental paternalism was put forward a generation after Hegel's death by J. S. Mill in his classic, *On Liberty*.

The second line of defense argues for a more positive defense of right. The right of recognition, as Hegel understands it, is not just a watery tolerance of others, adopting a hands-off attitude. It requires a more robust sense of respect for the "free personality" that is at the basis of right. At the basis of personality lies the idea of moral self-realization or self-development so crucial to Hegelian ethics. Here it is important to note that the personality is never something given but is always in the making. What sort of selves we become is always dependent on what sorts of activities we engage in. The specific institutions discussed in the *Philosophy of Rights* are intended to provide the necessary categorical framework within which our individual powers and capacities can grow and develop. Without such a categorical framework to provide some kind of moral ballast, our lives would threaten to become rootless, alienated, and anomic.

What I have called Hegel's positive defense of right is indicated in his decision to treat politics as a branch of ethics. Like Plato and Aristotle, he denies the possibility of an independent sphere of morality detached from politics and consequently an independent science of morality detached from political philosophy. Indeed, the Hegelian state is not neutral vis-à-vis the ways of life of its citizens. Its goal is the positive one of promoting a form of *Sittlichkeit* in which all citizens can share. The division of ethical life into family, civil society, and the state is a form of social differentiation that seeks to imbue citizens with some sense of esprit de corps, or common purpose (1972, 133; *Werke* 7:359). In the final analysis, then, Hegel's political program is a form of civic education or *Bildung*.

Many interpreters have seen in Hegel's theory of *Sittlichkeit* an incipient relativism according to which standards of right and wrong can only come from existing conventions and institutions. His well-known claim that "every individual is a child of his time" and that a philosopher can no more transcend his age than "an individual can . . . jump over Rhodes" is often taken as evidence for his relativism (1972, 11; *Werke* 7:26). Likewise, his identification of *Sittlichkeit* with "absolutely valid laws and institutions" and "habitual practice" appears to give it a conservative dimension similar to Burke or any apostle of traditionalism (1972, 105, 108; *Werke* 7:294, 301).

But this is to misunderstand. Hegel's theory of *Sittlichkeit* is not just an empirical, sociological description of what institutions happen to exist; it is a rational reconstruction of what institutions *must* exist if rational freedom is to be possible. Institutions and practices are not in Hegel's philosophy called upon to be the judges in their own case. Rather they are judged by their capacity to further and sustain our mutual desire for freedom. Hegel's idea of freedom is tied to an evolutionary or progressive theory of history, the culmination of which is the modern constitutional state. Only in the institutions of the modern constitutional state does one find the kinds of practices and institutions that embody "the actuality of concrete freedom" *(die Wirklichkeit der konkreten Freiheit)* (1972, 160; *Werke* 7:406). In the rational state, now coming into being, the conflict between philosophy and politics will cease to exist. In such a state institutions will be arranged to express every facet of a developed human intelligence and the "plain man," like the philosopher, will live in a condition of mutual trust and respect.

Conclusion

Two objections stand in the way of an endorsement of Hegel's theory of right today. The first is that whatever undoubted merit Hegel's arguments may have, they are simply outside the U.S. context, where there has never been a strong state-centered tradition as required by Hegel. In Hegelian terms the United States has evinced the power of civil society over the state. U.S. attachment to liberalism, especially in its Lockean forms, has prohibited the development of a more robust sense of "the political," which Hegelian politics seems to require. Accordingly, the creed of unbridled individualism has been virtually the one "self-evident" truth shared by most U.S. citizens. "The reality of atomistic social freedom," Louis Hartz proclaimed in his magisterial *Liberal Tradition in America,*" is [as] instinctive to the American mind, as in a sense the concept of the polis was instinctive to Platonic Athens or the concept of the church was to the mind of the middle ages" (Hartz 1955, 62).

Without dwelling on the vexed question of the role of Lockean liberalism in defining U.S. national character or the adequacy of Hartz's depiction of Locke, it is at least arguable that another more "Hegelian" conception of statehood and political development has been at work in our tradition ever since the founding. According to Samuel Beer, the United States is and has been since 1787 not just a collection of semisovereign states united for the limited purposes of security and prosperity but—to use the language of Daniel Webster—a genuinely "national community" where "liberty and union" are "one and inseparable." By *a community* Beer means first of all "an emotional fact: a massive background feeling of 'belongingness' and identification." On such a view "we are joined with a vast national community by a distinctive kind of emotional tie: by *public* joy, grief, pride, anger, envy, fear, hope, and so on" (Beer 1967, 165). Among those who have invoked the national idea have been Alexander Hamilton, Daniel Webster, Abraham Lincoln, and, in our century, Theodore and Franklin Roosevelt. The national idea is one that cuts across party cleavages and unites Federalists, Whigs, Republicans, and Democrats. And in a subsequent article Beer argues that our most important political task today is "to keep alive in our speech and our intention the move toward the consolidation of the union" as opposed to a destructive particularism (Beer 1982, 23–29).

A second and perhaps more formidable objection to Hegel runs as follows. Even granted the persistence of certain consolidating or "Hegelianizing" tendencies in our tradition, it does not follow that the national idea represents some kind of historical absolute as Hegel thought it did—a final reconciliation of reason and reality. It is just this metaphysical interpretation of the state, so this objection runs, that condemns to irrelevance whatever apparent merits Hegel's arguments may have. One could argue, as some of Hegel's defenders do, that Hegel's insights can be saved only by disentangling them from the skeins of his speculative metaphysics and philosophy of history. But to be consistent, one would have to admit that Hegel's depiction of the modern state as the crowning apex of world history is simply wrong.

This objection need not be accorded the last word. In the first place, this objection is often premised on an alleged inconsistency in Hegel's thought. His attempt to portray history as a completed (or completable) process moving toward a final telos is said to betray the dialectical element in his thought with its endless negativity and rebellion against all fixity. The true Hegel is not the conservative idealist but the revolutionary dialectician for whom "overcoming" and "self-transcendence" are all that matter.

The claim that Hegel arbitrarily arrests the dialectic, forcing it to culminate in the present, is flawed. Here I can only say that the distinction often drawn between dialectic and system, methodology and metaphysics, is entirely foreign to Hegel's thought. This was a distinction foisted onto his thought by latter-day disciples seeking to put his dialectic into the service of various radical causes. Hegel's dialectic is more concerned with the "mediation and overcoming" (*Vermittlung und Aufhebung*) of conflicts than their intensification. The crucial role assigned by Hegel to these concepts is lost if we persist in regarding the dialectic simply as the power of the negative and see all societal forms as so many varieties of unfreedom. The Hegelian dialectic is concerned with the resolution of contradiction by means of speculative reason. It is my contention that far from being at odds with his politics, Hegel's dialectical logic is profoundly consistent with the ethical community sketched out in the *Philosophy of Right*.

Second, it would be overly hasty to dismiss Hegel's thesis about an end of history as an antiquated metaphysical prejudice left over from an age of faith. If we understand the end of history to mean a condition characterized by an overall consensus on the ends of life, we can see that it bears an uncanny resemblance to another movement of modern thought, the "end-of-ideology" thesis proclaimed by several prominent U.S. intellectuals in the 1950s and 1960s. The proclamation of an end of ideology assumed that the passions that had generated the political fanaticisms of the past were now spent and that the imperatives of attending to the postwar industrial economy would form the basis for a new consensus. This consensus would not just be another ideology but would be an anti- or counterideology where individuals would agree to resolve their differences in a more pragmatic, piecemeal manner without recourse to grand principles.[7]

I take the end-of-ideology thesis to be self-refuting for the same reason that Hegel's end-of-history thesis is. Far from making an end of history, Hegel's thesis was itself a notable expression of the history of his own time and place. Hegel was not the first—and will certainly not be the last—philosopher to succumb to the temptation of endowing his thought with a permanence and validity that he denied to others. This is not to make the obvious point that Hegel underestimated the peculiar limitations of the time and circumstances in which he wrote. The point is that if Hegel was correct when he said that every philosopher is a child of his time and that philosophy is "its own time apprehended in thoughts," his attempts to insulate his philosophy from the process of historical change that he so brilliantly analyzed could not but meet with failure.

Hegel was, I believe, profoundly correct to see in history a rational process where great and liberating ideas become impediments to the development of future thought and thus unwittingly provoke their own demise. When applied to itself, Hegel's end-of-history thesis could not but become another orthodoxy that in time would generate its own antithesis, namely, an end to the end of history.

Critical theory, deconstruction, and hermeneutics represent but three candidates to succeed Hegel. Postmodern critics like Jacques Derrida and Jean-Francois Lyotard have seen in Hegel's monumental "system" with its periodization of history into distinct phases of spirit nothing but a thinly veiled attempt to gain control over the past and thereby to dominate the future. In place of Hegel's synthesis of reason and history, postmodernism claims to offer no new philosophical system or "grand theory" but rather a "hermeneutics of suspicion," a perpetual watchfulness over the self-professed purveyors of schemes proclaiming emancipation and enlightenment.

The question, then, is what can be retained of Hegel's progressive philosophy of history once it has been submitted to the assaults of postmodern skepticism? One answer could be, a more supple or provisional notion of an end of history.

"Every historian," Jürgen Habermas has written, "is in the role of last historian" (Habermas 1977, 350). This is to say that we must regard our own epistemic standards and norms of rationality not as absolute in some transcendent sense but as binding on us at least until something better comes along. Unless we are prepared to give up altogether the idea of gaining a critical purchase on history, we are compelled to judge it from some kind of absolute standpoint. Such a standpoint need not be metaphysically grounded but can perhaps be discovered immanently or pragmatically in the forms of human discourse. There is, as Habermas has suggested, a telos of agreement implicit in our very use of language. In any case I take this to be a worthwhile task for the political philosophy of the future.

Notes

Earlier versions of this paper were presented at the 1987 annual meeting of the American Political Science Association in Chicago and at an international colloquium at Yale on Hegel and Hölderlin. I would like to thank the participants in Yale's Political Theory Workshop for helping me to clarify some of the arguments.

1. Throughout this article I cite both English and German editions of Hegel's works. The German edition to which reference is made is Hegel 1971b, identified as *Werke* and followed by reference to volume and page number.

2. For many of the debates surrounding the political intention of the *Philosophy of Right* in the years immediately following its publication see the collection edited by Riedel (1975). According to N. von Thaden, H. E. G. Paulus, and the anonymous reviewer for the *Allgemeine Literaturzeitung*, Hegel was a conservative defender of the status quo for whom the Prussian monarchy represented the actuality of reason (Riedel 1975, 63-64, 76-77, 153-57). The liberal defense of Hegel was left to his erstwhile student and later editor Eduard Gans (Riedel 1975, 242-48) for whom the state depicted in the *Philosophy of Right* was a constitutional system where monarchical power was limited by law and, contrary to prevailing Prussian practices, legal and juridical proceedings were to be public. Out of these early debates grew the divisions between the Left and Right Hegelians of the 1840s and 1850s, which still have their contemporary analogues. For an exhaustive treatment of the whole subject see Toews 1980.

3. For work that attempts to defend a similar position see Stillman 1974 (pp. 1086-92); Hinchman 1984 (pp. 7-31); Smith 1986 (pp. 121-39).

4. For useful treatments see Shklar 1971 (pp. 83-87); Steiner 1986 (pp. 19-42).

5. The chief expositor of this view is, of course, Kojève (1947); for an able critique see Kelly 1978 (pp. 29-54).

6. For a view of Hegel along these lines see Nisbet 1966 (pp. 54-55).

7. For a sample of this view see Aron 1957, Bell 1960, Lipset 1959.

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Steven B. Smith is Associate Professor of Political Science, Yale University, New Haven, CT 06520.

TIME AND POWER IN AFRICA

HENRY BIENEN
NICOLAS VAN DE WALLE
Princeton University

Both rapid leadership turnover and remarkably durable leaders can be found side by side in African systems of personal rule. In order to explain differences in time in power among African leaders we employ life tables analysis and hazard models, a multivariate technique. We find that the risk of losing power is a decreasing function of time that is little affected by country or leader particularities. The best predictor of whether a leader will lose power in any given period is the length of rule up to that point.

We will map and explain an observable pattern of African leaders' risk of losing power by putting time as a variable at the center of an analysis of instability. Many studies examine leadership styles and personal characteristics of leaders. Others analyze leadership turnover by focusing on the social, political, and economic characteristics of countries. We try to see how much variation in leadership turnover in what are mostly nonelectoral systems can be accounted for by treating time in power as an explanatory variable. Time in power has its own effects. It captures variables that in practice we cannot measure independently, such as leaders' ability to build political networks and to acquire and use information.

African politics for more than 25 years has been characterized by frequent regime changes, often accomplished by military coups. This state of affairs has been accompanied by the duration in power of some African leaders. Habib Bourguiba of Tunisia, Felix Houphouet-Boigny of the Ivory Coast, Julius Nyerere of Tanzania, and Kamuzu Banda of Malawi, dominated their national political systems for over two decades. Indeed, 11 of 51 African nations (including Zanzibar) knew only one leader from independence through October 1987.

Africa gives us examples of rapid turnover of leaders and leaders who stay in power for very long periods. Furthermore, a country may have a great deal of leadership instability and next have lengthy rule of a single leader. This pattern has characterized Benin, Togo, and Zaire. Also, some leaders have stayed long in power, ostensibly ruling through a single party, to die in office, leaving leadership instability followed by the disappearance of the ruling institution. This occurred in Guinea after the death of Sekou Toure in 1984. An individual may stay in power over an extended period with a great deal of concurrent regime instability. This was true for Mobutu in Zaire, Stevens in Sierra Leone, Nimeiri in Sudan, to name only some. This phenomenon makes us consider what we mean by rule, since an African leader's maintenance in power may be over a narrow realm of people, territory, and processes.

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A ruler's writ in Africa, enforced by repression, may not carry all that far. (Bienen 1970; Zolberg 1966).

Our analysis deals only with one aspect of rule and of instability. We are interested in duration of leaders, their length of stay in power. We do not analyze regime instability. In order to show that time itself is a critical explanatory variable, however, we must consider how important the characteristics are that pertain to leaders and to countries and that are usually used as independent variables to explain stability outcomes.

All politicians have resources and abilities. They operate within contexts that provide opportunities and constraints, bases of support and opposition. African politicians typically have communal, territorial, institutional, occupational, class, and factional bases as do their counterparts in other regions. Some came to power with already-established political networks because they led an anticolonial struggle. Others built these networks after independence. Some were young on assumption of powers. Others were well into middle age or beyond. Our investigation inquires into the importance of the age and background of leaders.

We are also interested in whether such characteristics of countries as colonial experience, rates of growth, and degree of ethnic conflict are significant for explaining length of time in power.

We consider this analysis to be exploratory for a number of reasons. First, there are difficulties in specifying some of the country characteristics that might be significant for leadership duration. Second, the sample size of 165 creates problems for the analytical techniques we employ in analyzing the conditional risks of losing power within annual intervals. We bring to bear both life table analysis and multivariate hazard models. These techniques should give more robust results with a larger sample. We plan to extend our sample to other parts of the world.

Even within the limits of our present analysis, we get interesting results. Neither variation in country characteristics nor variation in personal characteristics alters a general pattern of decreasing risk of staying in power over time.

We started off with the hypothesis that leadership duration would be graphed as a U-shaped curve; that is, we assumed that many leaders would fall from power within a fairly short time period, say, one to three years. We assumed that there would be a threshold affect for staying in power. After a certain time, leaders would accrue resources, have knowledge of their system and their enemies, put together coercive networks and be able to stay in power for some extended period. We assumed that although we could not independently measure leadership skills, leaders with poorly developed or the "wrong" leadership skills would fall by the wayside. Thus there would be a weeding-out process.

We further hypothesized that as leaders aged, their abilities to wield the instruments of power would weaken. Our defining example was Haile Selassie, who was a master at court politics but whose abilities to manipulate the politics of the court declined in old age, quite apart from the pressures that worked in on him (Kapuscinski 1983). Of course we knew that death comes to survivors in power, as it did to Jomo Kenyatta in Kenya at an advanced age and Agostinho Neto of Angola in middle age. We presumed that at the start there might be some effect—analagous to a threshold effect for retaining power—where at some advanced age leaders might fail more often. (On aging see McIntyre 1983). Or, conceivably, there would be some length of time in power after which opposition to leaders might strengthen.

To anticipate our findings, we show that the U-shaped hypothesis is not borne out. Risk of losing power does not increase for African leaders who are very old and have been in power for more than two decades. While the level of risk does not decline monotonically, it settles after seven to nine years at a level a little higher than for four to six years but half as high as for zero to three years. We will see that the length of time a leader has held power is the best predictor of how long a leader will continue to hold power.

Leaders face a high risk in the first years of their rule. Risks of losing power then decrease sharply. This pattern of risk turns out to be robust vis-à-vis a large number of both country and leader circumstances. The relationship holds for both civilian and military rulers and is little influenced by the manner with which the leaders achieved power, by the presence of ethnic conflict in the country, or by the country's colonial background. Furthermore the relationship applies regardless of a country's economic growth record. Although social, economic, ethnic, and institutional factors may influence the absolute risk levels of losing power in any given year, they do not alter the general pattern of decreasing risks. Indeed, old age does not alter this pattern either. Even elderly leaders have decreasing risk of losing power once they have been in power for a long time, albeit they are more likely to die of old age.

The only subsample of leaders that appears to face a different pattern of risk are those who emerged as rulers at the outset of independence. These leaders had very special resources for rule as founding fathers.

Analytical Methods

Our analysis applies models derived from survival analysis and used in demography and economics. We did fit different classes of models to our data but the models were arrived at independently and deductively. Survival analysis employs hazard models, which are multivariate techniques that estimate the effect of a variable while holding constant the effect of the model's other variables.[1] Survival analysis provides estimates of the risk of an event occurring during different time intervals and makes possible estimates of the effects of specific factors on that risk.

Survival analysis allows for censoring, that is, for the existence of subjects not observed for the full period until the occurrence of the event of interest. A problem occurs when a study ends before the event of interest occurs. Some African leaders died of natural causes or were still in power at the end of the sample frame. Excluding these subjects from analysis typically introduces bias and we need a method of handling them. (Miller 1981, 2-5, 22-25).

Rulers still in power through October 1987 were treated as censored data. We defined natural deaths, which occurred in 13 cases, both as a loss of power and as a censored observation to see whether this made a difference. It did not. The results presented treat death from natural causes as censored observations because such deaths are treated as independent of the event occurrence—a loss of power from political causes. Assassinations were not treated as censored observations.

Since we are interested in the political ability to stay in power, and not in the physical one, it makes sense to treat deaths from natural causes as censored observations. On the other hand, aging may impinge on a leader's ability to sustain a grip on power, in which case natural deaths should not be censored, as this would introduce a bias in the results. The results from censoring natural deaths or not were similar.

We fit three classes of hazard models to our sample. The first class might be termed a "constant hazard model" because it embodies the assumption that the risk of losing power is a constant function of time. Different characteristics of leaders or countries have positive or negative effects on risks of staying in power independently of time in power. We estimated several "constant hazard models" from single variable models to one with all the defined variables.

A second class of models might be called "proportional hazards models." They retain the assumption that explanatory variables have a constant effect over time, but they allow the hazard to change in each of a discrete number of duration intervals and in fact, rather than estimate one underlying hazard for the entire period, they estimate one for every time period. The different covariates then have a constant positive or negative effect on each time-specific underlying risk. To estimate such models, discrete time intervals must be chosen. We divided the time distribution into six intervals: entry-into-power to 2 years, 2 to 4 years, 4 to 6 years, 6 to 9 years, 9 to 13 years, and more than 13 years. This reflected our attempt to capture the critical periods in the time distribution as parsimoniously as possible to maximize the number of observations per cell.

In a third class of hazard models, explanatory variables are allowed to have different effects on risks in each of the estimated time periods. We call these models "time-dependent hazard models" because they allow the possibility of interaction between time and specific variables. For example, we wanted to test the hypothesis that the age of a leader might have a different effect on the risk of losing power in the first two years of rule than in the sixth to ninth year. The "time-dependent" hazard model allows us to do this.

Several models with time-dependent explanatory variables were estimated, to allow a comparison with the proportional hazards class of models in order to determine whether or not the assumption of interaction between time and covariates is appropriate.

We also used life table analysis. Life tables are a univariate class of hazard models. They employ conditional probability techniques to describe the longitudinal distribution of specific discrete events, experienced by homogeneous groups of subjects, based on the notion of exposure, or total time at risk.

The existence of any unmeasured background factors influencing survival will violate the necessary assumption that all subjects in each group have the same distribution of survival times. We cannot be sure that other background factors influencing survival are absent without resorting to multivariate analysis. We do this using the hazard models.

In the life table analysis one covariate that could not be tested was the time period itself. We would like to make sure that our results are not confounded by the existence of certain periods between independence and 1987 during which there was a high degree of leader turnover. For example, it might be hypothesized that the levels of commodity prices in specific periods have effects on leaders' strength. Such period variables cannot be specified adequately in life tables analysis. But the multivariate hazard analyses allow us to report on the significance of the date at which leaders came to power.

Thus we resorted to several types of hazard models to test for confounding factors in order to seek confirmation for the findings of our univariate life tables analysis. Hazard models also allow the possibility of identifying and estimating the pure effect of time on risks. We thus estimated several hazard models to test our initial hypothesis that the risk of losing power is a decreasing function of time that is not significantly altered by any of the country and leader characteristics.

The Data

Our data comprised 165 postindependence African leaders from 51 nations, including North African and Indian Ocean states. Our sample consisted of Organization of African Unity member states. South Africa has never been a member and was not included. Morocco was a member during the period of study and was included. The first leader of post-independence Zanzibar, the sultan Bin Abdallah, was also included. We defined a leader as someone who reached power with the ambition to stay there, regardless of the length of actual tenure. Interim heads of state were excluded; for example, we did not include Jerry Rawlings's first stint as head of the Ghanaian government in 1979 or Siwar Dahab's rule in the Sudan in 1985, since both set and kept time tables for withdrawal from power. On the other hand, we did include David Lansana of Sierra Leone even though he lasted less than a week, as it was his ambition to remain in power.

Our time frame was from independence through the end of October 1987. For the countries that had not been colonies as late as the 1950s, we made the following partially arbitrary decisions: for Liberia we began with Tubman in 1944; for Ethiopia we started with Haile Selassie in 1930; for Libya we began with Idris in 1951 and for Egypt we started with Fouad in 1923. In some cases (Obote in Uganda, M'ba in Gabon, Maga in Dahomey, Mahdi in Sudan, Mobutu in Zaire, Dacko in Central African Republic, Maghoub in Sudan) a leader reappeared after losing power. We treated such a one as a new leader on the second assumption of power. (A full list of leaders and their characteristics is available from the authors).

Variables were defined in terms of leaders' characteristics and countries' characteristics. Leaders were divided between those who had used extraconstitutional means to achieve power and those who had resorted to constitutional mechanisms for attaining power, in order to see if there were legitimacy effects on leaders' durability. Leaders were divided between those with and without a position in the national armed forces at the time they entered power, since military or civilian backgrounds divide African leaders. (Obviously, there is overlap between military leaders and those who use extraconstitutional means to achieve power.) In the life table analysis leaders were divided according to whether they were 45 or older or were under 45 – the median age of all leaders on assuming power. In the hazard model analysis we treat age as a continuous variable. The sample was divided between those who were the first leaders of each country at independence and those who were not first leaders.

Countries were divided into states with high, middle, and low levels of ethnic conflict, based on our own rankings. The sample was divided according to colonial history, comparing both the French and British excolonies with the rest of the sample. The sub-Saharan African countries with a colonial background were also distinguished from the rest of the sample. Countries were grouped according to the percentage of the population that was Islamic.

Countries were divided by both economic growth rates and income levels. Income levels were specified by the World Bank's country classification of low- and middle-income countries as of 1973 to divide the sample. These variables – ethnic conflict, growth rates, colonial background – are conventionally used in cross-national aggregate data studies that investigate regime stability.

Specification of the variables is not without problems. For example, per capita GNP does not adequately capture the full cross-national variation in purchasing power or income distribution, and using the 1973 country classification may introduce a bias for the leaders of countries that have changed income categories over the period of study. Per capita GNP figures for 1980 were also tried as an explanatory variable instead of the 1973 figures but proved to be a less satisfactory estimator. The importance of the latter problem is reduced by also differentiating countries by growth rate. The sample was divided into leaders of states that averaged at least 2% annual per capita GNP growth from 1965 to 1983, on the one hand, and leaders of states with lower growth rates on the other.

Results of Analysis

Studies of leaders often try to isolate their skills and the political resources available to them in order to analyze the gaining and retaining of power. This may be done by focusing on social backgrounds of individuals or personalities within regime types or regions. Such studies relate leaders' characteristics to structures of leadership and political systems (Blondel 1980). Studies may focus on leaders' styles and personalities as the cause of strategies undertaken (Cartwright 1983; Jackson and Rosberg 1982; Wriggins 1969).

It is difficult to isolate leadership skills and the political resources available to a leader in order to explain duration in power because such skills and resources have to be measured independently, not deduced from duration. Indeed, if we had completely independent measures of leadership skills, not ex post facto measures, we could test the hypothesis that in weakly institutionalized African political systems a leader's duration in power depends on how well that leader wields patron-client networks and uses carrots and sticks to reward followers and penalize opponents. We could test whether or not the duration in power of African leaders is really a selection process over time among heterogeneous leaders.

If the population of African leaders is made up of the skillful and unskillful, we would expect a high turnover rate in the first several years as the less skilled are ousted from office. After 10 years duration in office, perhaps only the skilled would remain. Thus, differences in leader duration might in no way be related to special difficulties associated with the first years in power, although we have reason to think that these years are more difficult. Instead, it may be true that it is *equally* difficult for leaders to stay in power regardless of how long they have been in power, but by the time they are past the first few years all of the unskillful leaders have simply been ousted. This is the type of process that demographers describe concerning *child conception:* there may be an equal probability of conception every month, but we observe a drop in the probability of conception for nonpregnant women as time goes on because those most likely to conceive do so right away and are selected out of the population.[2]

We cannot test for this hypothesis without making independent judgments about the skills of African leaders. It is interesting however, that leaders not necessarily thought to be very clever before assuming power like Moi of Kenya, Eyadema in Togo, and Kerekou in Dahomey-Benin, have stayed in power many years. Much admired and respected men have fallen by the wayside after short stays in power. Perhaps they were not skillful at the tasks of rule, but perhaps they failed despite skill.

However, we do not examine leadership styles and abilities. Nor do we try to add to the cross-national multivariate analyses that endeavor to account for the incidence of regime changes, especially coups, as a function of the socioeconomic and political characteristics of systems, although we do examine some social and economic correlates of leadership duration.[3] The structural weaknesses of all African political systems account in part for patterns of governance in these countries, including the resort to military coups. But cross-national regressions using coups as a dependent variable tell us little regarding when leadership changes occur.

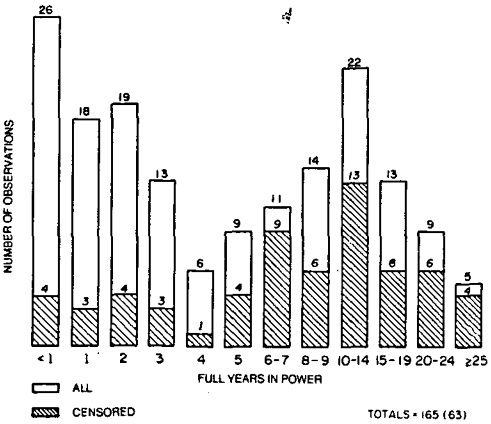
Our focus is on a leader's loss of power. We examine the risk, or hazard, of losing power in a given year over the course of the leader's tenure in power. In addition, we test whether or not various factors described below alter the pattern of risk leaders faced.

Life Table Analysis

Figure 1 presents the overall sample of 165 leaders in 51 countries. It is clear that many leaders are in power a short time and that a few leaders remain a long time. The median time in power is 5.1 years. If we take into account censored observations, the median is 8.6 years.

It appears from this figure that the distribution of time in power follows a decreasing curve but one that does not decline monotonically. However, the large number of censored observations prevent any clear-cut conclusion without resorting to life tables.

Figure 1. Distributions of Time and Power



Note: Censored observations include leaders who died of natural causes while in office.

Figure 2. The Conditional Probability of African Leaders Losing Power by Year (Hazard Rate)

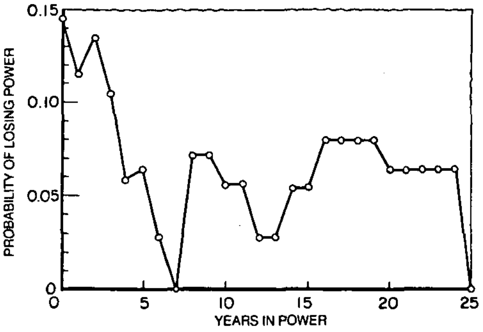
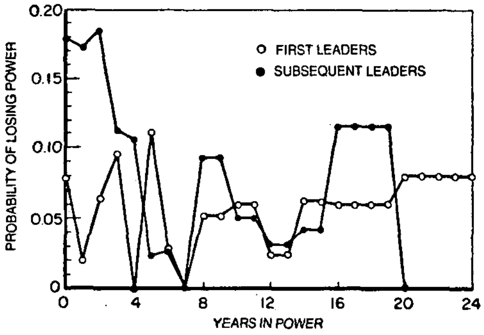


Figure 2 shows a hazard function for the entire sample. It provides estimates of the conditional risk of losing power within annual intervals, once censored values are taken into account. The conditional risk of losing power in a given year is at its peak in the first three years and then decreases dramatically. Twenty-six of the 165 African leaders between independence and 1984 survived less than a full year in power and more than a third were ousted within three years of their entry into office (Figures 1 and 2).

At the outset of our study we had hypothesized that the conditional risk of losing power might increase after a certain time in power. Leaders might overextend themselves or their political resources would be of decreasing value in the face of economic mismanagement, the inability to process increasing demands, or simply old age. For these reasons the pattern of risk for African leadership might be hypothesized to follow a U-curve. Interestingly, the full data set does not support this hypothesis, as the hazard rate does not significantly increase at any point beyond the first few years, allowing for statistical irregularities.

We constructed a set of life tables to compare subsets of leaders. Each covariate except colonial background was repre-

Figure 3. The Conditional Probability of First and Subsequent African Leaders Losing Power by Year (Hazard Rate)



sented by a dichotomous variable: high or low ethnic conflict, fast or slow GNP growth, middle- or low-income country, large or small Moslem population, first or subsequent leaders, younger or older leaders, constitutional or extraconstitutional entry, civilian or military leaders. Colonial background was divided into three categories: French, British, and others. In this case, one category was tested against the other two. Nine covariates provided 19 subgroups from the sample of 165 leaders and captured much of the variation in risk patterns.

Survival functions for the different subsamples of leaders indicate that each subsample undergoes the same convex curve, with a gradually decreasing slope through the period. The curves appear broadly similar for all of the subsamples except for the first leaders—the group of leaders who came to power at the end of colonial rule and who have a higher survival rate—and the group of leaders from high-ethnic-conflict countries, who appear to have a lower survival rate. This result was confirmed by the log-likelihood statistics, which compared each subgroup with its control group.[4] At the 95% level of confidence (i.e., P value of less than .05), the control group that appears significantly different is first-leader category.

Leaders who reached power in a manner not sanctioned by the national constitution appear to face the highest risk in the first two years. Thenceforth this subgroup appears no different from the other subgroups.

Military leaders follow a similar pattern, as might be expected since 60 of the 67 military leaders in the sample achieved power through extraconstitutional means. Leaders entering power through extraconstitutional means appear to suffer the highest risks of losing power rapidly, reflecting perhaps the initially low level of legitimacy of their rule. In some instances—for example, Jerry Rawlings in Ghana or Gaafer Nimeiri in Sudan—broad dissatisfaction with previous civilian rulers provided a degree of legitimacy in the early years and enhanced the leader's abilities to build up the necessary political networks and secure power. This effect does not appear to have benefited most military leaders. In general, hazard levels observed underscore the fragile political base on which military leaders typically make their move for power. Few of them can count on the undivided support of their colleagues in the armed forces, as evidenced by the number of countercoups occurring within the first few years of a military takeover.

The first-leaders subgroup commands special attention because it clearly does follow different hazards and survival patterns. Leaders who emerged from the anticolonial struggle appear to face lower risks in almost every time interval; more importantly, they faced much lower risk levels in the first years of rule and their highest risk comes in the fifth year, later than for the other groups. Such a different pattern for first leaders suggests they may constitute a confounding factor for the full sample. In Figure 3 the annual risk of losing power is graphed for first and subsequent leaders. The pattern of decreasing risks becomes even more pronounced for the latter.

The first leaders may have had special legitimacy, for a while at least. They had patronage appointments to make that were created by the transformation of colonial structures into national ones and the consequent rapid Africanization of personnel. Still, first leaders grasped power in different circumstances, and they were from different backgrounds. Advantages accruing to them differed widely.

There were traditional leaders, such as Emperor Haile Selassie and Mohammed V of Morocco, who brought to power the moral and religious authority of their positions and persons. This category of leader is not simple to define, however. Traditional leaders sometimes possessed only limited regional or ethnic legitimacy and their claim to national power was tenuous or unsuccessful.

A second category of first leaders was composed of the nontraditional leaders of African states. Generally, they benefited from the prestige they had accumulated during the anticolonial struggle, but they grasped power under circumstances that varied greatly. Some had built up strong political networks before independence through a nationalist movement that later became a ruling party, as did Touré of Guinea, Keita of Mali, Houphouet-Boigny of the Ivory Coast, Nyerere of Tanzania, Nkrumah of Ghana, Senghor of Senegal. The strength of these leaders should not be exaggerated with the advantage of hindsight. The political machines they rode to power were rarely well organized and few of them enjoyed mass support after independence. Within the group of leaders with strong networks at independence, most continued to face strong opposition. Nonetheless, the political base of "strong network" first leaders upon assuming power has rarely been replicated by leaders subsequent to them.

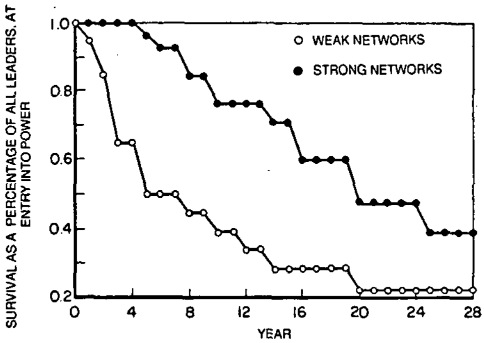
Many first leaders came to power without strong domination of political networks. While having great stature, such leaders were often out of the country or in jail for extensive periods and only came to dominate political life on their public reemergence in the months before full sovereignty. Jomo Kenyatta belongs to this category. Other leaders seemed strong but their leadership was fragile, such as Ben Bella in Algeria. Banda of Malawi is harder to place. Out of the country for 40 years and, like Kenyatta, relatively old when he returned to a tumultuous welcome in 1958, he rapidly developed the backing required to dominate political organizations by independence in 1964.

A third category is provided by the Portuguese colonies and by Zimbabwe. Here, country and leader characteristics merge because the independence process was marked by protracted guerrilla struggle and the leaders who triumphed had been either military commanders or had developed strong party-military ties. In addition, some of these political-military organizations administered territories under their control during the anticolonial struggle.

Three categories of first leaders—the "traditional monarchs," "founding fathers," and "guerrilla leaders"—can be subdivided further into our categories of leaders who had relatively strong and relatively weak networks and resources. A number of factors go into our judgment of networks. For example, the Convention Peoples Party in Ghana provided a network across Ghana although it was stronger in some regions than others. Nkrumah had opponents in the CPP but he had a very strong position within it and knew a great deal about political cleavages in Ghana. Houphouet-Boigny appeared to have an even stronger organizational base in the Ivory Coast. Both would be strong network leaders.

Figure 4 graphs the survival distributions of the two sets of first leaders. The curve for strong network leaders domi-

Figure 4. Survival Rates of First Leaders with Weak and Strong Networks



nates the one for weak network leaders throughout the time frame. The result is more impressionistic than rigorous, but the evidence nonetheless suggests that for the first leaders in all categories, the political base that individuals brought to office did have a bearing on their ability to sustain a grip on power once the grace period after independence ended.

We need to investigate the possibility that the patterns of risk we have found reflect variations among countries rather than among leaders. The former is made plausible by the existence of countries with many leadership changes, contrasting sharply with those nations which have known only one ruler since independence. We do not want to overlook the possibility that certain kinds of countries have a greater propensity for leadership change than others.

Life table analysis finds essentially the same pattern of risks for leaders irrespective of colonial ruler. The pattern of maintenance in power appears the same for the French, British, and Portuguese ex-colonial subgroups. The other variables that described country characteristics proved to be of greater significance in explaining the variance of risks faced by leaders. The level of per capita income appears to be a more relevant factor than the rate of GNP growth, using the log-likelihood statistics to compare the two distributions. As noted, neither variable was specified with the desired level of precision, and the results must be treated with caution. Leaders in countries with a higher economic growth rate or income level appear to face a lower risk of being pushed out of power throughout the time distribution, although the risk differentials follow a clear pattern only in the first few years of rule.

North African countries have greater leadership duration compared to countries in other regions in Africa. This may result from their generally higher national income levels. On the other hand, the North African leadership stability might suggest that Islam is a critical factor in affecting leaders' survival. We tested this by taking countries that were over 60% Moslem and compared their leaders' risk of losing power to leaders' risks in countries that were less than 60% Moslem. The resulting two life tables were not statistically significantly different.

Examining the life tables of leaders in the two ethnic conflict categories, we observe differences of similar orders of magnitude. Leaders in countries with high levels of ethnic conflict face consistently higher hazard rates except in the third year. The tests that leaders face seem to come slightly later when levels of ethnic conflict are relatively lower.

One way to test whether a country is particularly difficult to rule is to correlate a leader's duration in power with the duration in power of the leader's predecessor. In the African sample of 114, the "r," or correlation coefficient, was .007, indicating insignificant levels of correlation. The first observation for each country is lost since first leaders did not have predecessors. And there is no way to handle censored observations for the leaders still in power today. If a country has had only one leader, we have no observation for it. Thus we do not put great store in this correlation.

We are not prepared to argue on the basis of this sample that the relative risks of losing power for one leader in a particular country in any given year are no different from the risks faced by leaders in other countries. Rather we are arguing that the total African leadership pattern of risk over time is little influenced by country-specific factors and that the pattern of risk is a decreasing function of time in all countries.

Effects of Time

We employed multivariate hazard models to analyze the effects of time variables on leaders' risk of losing power. A variable for date of leaders' entry was used to test for the possibility that certain periods in history have been more or less hazardous. Secondly, the age of leaders was specified as a continuous variable. Thirdly, we evaluated the effect of duration in power itself.

Age and date of entry proved to be statistically insignificant. Older leaders, it might be thought, would have had a longer time to develop political networks before entering power and would therefore face lower risk levels in the early years of their rule. Over time, however, they might be more likely to lose the energy required to continue to dominate national life and beat back opposition. Neither proposition is confirmed.

We estimated three proportional hazard models. One has all the variables of interest (Table 1, col. 1). One adds age to all periods (Table 1, col. 2). And one, which we call the "preferred model" is more parsimonious (col. 3): it estimates only the four most powerful explanatory variables from the full model.

Of interest in these models are the "duration" coefficients for each of the specific periods. These are estimates of the underlying effect of time in power on hazards, net of the estimated effect of the different covariates. Examining these estimates, we confirm a pattern of decreasing risks over time. The highest annual risks are reached in the second period (two to four years in power) and followed by a clear decrease through the last period. The estimates have to be divided by the number of years in the period to get the annual risks. Thus, for example, a coefficient of .24 in the first period suggests an underlying annual risk of losing power—net of the effects of all the explanatory variables—of .12. This initial increase in the underlying risk may suggest that the problems of less-skilled leaders being selected out of the sample may not be salient, at least not until after the fourth year of rule, at which point roughly half of our leaders have lost power or have been censored out.

As shown in Table 1 (col. 1) the dummy variables for sub-Saharan Africa and for first leaders are found to have strong explanatory power. The coefficients presented provide the percentage increase in risk due to the factor tested for, holding the others constant. Thus the first leaders faced 68% fewer risks of losing power than subsequent leaders throughout the duration of their rule. The results indicate that confounding factors may indeed be at work in the life tables. Military leaders is the only variable with explanatory power with 95% levels of significance. The economic variables, on the other hand, appear to have lost the significance they demonstrated in the life tables.

Variable time-dependent models were tested and compared to constant and proportional risk classes of models. The additional explanatory power achieved in the former by allowing effects to vary in each period are not significant, suggesting that the assumption of proportional effects over time is more appropriate for the data. National socioeconomic characteristics do not appear to be very important in determining patterns of survival rates, though a high degree of ethnic conflict or low income and growth levels correlate with greater risks. Our analysis does not support the idea that some countries are clearly more difficult to rule than others. It is true that we have not precisely specified ethnic conflict or economic growth in this analysis, though we doubt that better specification of variables would show different results.

A full-time dependent hazards model, with all of the explanatory variables is presented in Table 2. None of the variables are consistently significant throughout the different intervals, though the coefficient for the extralegal entry dummy, for example, is significant in the first period. The first leaders dummy alone appears to demonstrate a significantly different pattern of risk in the first years of power, with a sharp increase in risk in the second period. This model was compared with the full proportional hazards model of Table 1 through a log-likelihood test; no significant increase in explanatory power was recorded, suggesting again that there is little or no interaction between duration and the variables in our model.

Table 1. Proportional Hazards Models of Leader Duration in Africa

	Full Model (1)		Full Model with Ages (2)		Preferred Model (3)	
Independent Variables	Coefficient	F-Ratio	Coefficient	F-Ratio	Coefficient	F-Ratio
Extralegal entry	1.46	1.18	1.60	1.66	—	—
Date of entry	.97	3.54	.98	1.61	—	—
Military leader	.47	4.78*	.44	5.48**	.57	5.52**
Age at entry	1.01	.41	—	—	—	—
First leader	.32	13.93**	.37	9.27**	.36	15.57**
French colony	.89	.11	.70	.16	—	—
British colony	1.41	.14	.93	.01	—	—
Islamic population (%)	1.00	1.07	1.00	1.31	—	—
High-level ethnic conflict	1.39	1.80	1.29	.97	—	—
National income	.99	1.50	.99	1.60	—	—
GNP growth	.94	2.19	.92	3.40	.94	2.70
Sub-Saharan Africa	3.55	5.86**	3.40	3.40	2.80	6.59**
Duration 0–2 years	.24	2.14	.41	.44	.09	32.74**
Age at entry year	—	—	.99	.48	—	—
Duration 2–4 years	.26	1.78	.05	4.00*	.09	29.76**
Age at 2nd year	—	—	1.04	2.95	—	—
Duration 4–6 years	.15	3.45	.12	1.34	.05	35.44**
Age at 4th year	—	—	1.01	.02	—	—
Duration 6–9 years	.10	4.86*	.09	1.53	.03	40.22**
Age at 6th year	—	—	1.00	.01	—	—
Duration 9–13 years	.11	4.86*	.68	.03	.04	35.84**
Age at 9th year	—	—	.96	.94	—	—
Duration > 13 years	.18	3.07	.02	4.62*	.06	31.05**
Age at 13th year	—	—	1.04	2.34	—	—
−Ln likelihood	−310.79		−305.50		−317.94	
Degrees of freedom	(17)		(24)		(9)	

Note: The estimated coefficients have been exponentiated for convenience.

*$p < .05$.

**$p < .01$.

Table 2. A Time-dependent Hazards Model of Leader Duration

	Coefficients of Time in Power (in Years)					
Independent Variables	0–2	2–4	4–6	6–9	9–13	> 13
Extralegal entry	3.73 (5.17)*	.38 (2.02)	3.54 (.78)	1.95 (3.36)	.47 (.12)	.00 (.01)
Date of entry	.99 (.26)	.94 (5.68)**	.95 (.46)	1.03 (.24)	1.08 (1.18)	.85 (2.73)
Military leader	.35 (4.56)*	.82 (.08)	.03 (3.87)*	.05 (2.67)	1.40 (.02)	> 100 (.01)
Age at entry	.98 (.59)	1.06 (5.55)**	.96 (.84)	.98 (.18)	.91 (1.85)	1.04 (1.64)
First leader	.27 (4.50)*	1.41 (10.12)**	.13 (1.74)	.73 (.06)	2.09 (.16)	.11 (3.06)
French colony	.87 (.06)	2.63 (.99)	5.00 (1.39)	1.14 (.01)	.37 (.47)	> 100 (1.26)
British colony	1.67 (.68)	1.16 (.03)	5.17 (1.34)	5.29 (1.63)	1.51 (.07)	55.49 (1.44)
Islamic population (%)	1.00 (.45)	1.00 (.01)	.99 (.00)	.97 (2.41)	.99 (.08)	1.00 (.05)
High-level ethnic conflict	1.27 (.34)	1.16 (.09)	2.52 (1.27)	.10 (2.56)	5.36 (2.10)	2.76 (1.60)
National income	.99 (2.04)	.99 (.69)	.99 (2.12)	.99 (1.51)	.99 (.15)	1.00 (2.26)
GNP growth	.97 (.18)	.89 (1.55)	1.07 (.25)	.65 (4.43)*	1.04 (.04)	.91 (.26)
Sub-Saharan Africa	> 100 (.03)	1.57 (.24)	> 100 (.04)	.05 (2.09)	.50 (.17)	44.66 (1.20)

Note: F-ratios are in parentheses.
*$p < .05$.
**$p < .01$.

Conclusion

Results provide support for the hypothesis that few of the wide variety of country or individual characteristics tested have more than a very small effect on the risk of losing power, once the effect of time is taken into account. The group of first leaders alone provides evidence of an appreciably different pattern of risks and appears to confound the effects of other variables, particularly the economic variables. The hazard models we estimated provide evidence that the risk of losing power is a decreasing function of time that is little affected by country or leader particularities. Length of time in power captures variables that we cannot measure independently.

The best predictor of whether a leader will lose power in any given period is the length of rule up to that point. We have seen that leaders require time to consolidate power. First-generation leaders had advantages that subsequent rulers did not have. Partially, these advantages are explained by networks developed before independence and the legitimacy that accrued to being the "father of the country." Extended length of time in power allows for building and wielding networks of information, repression, and patronage. We can assume that leaders become more skillful at ruling over time. This is not the same as the assumption that leaders are more or less skillful at entry into power. It may be that leaders are heterogeneous by skill and that the less skillful fall from power early. However, even if this were true and leaders start from different places on a knowledge and skill curve, some unskillful leaders survive and move up the curve.

It is also true, of course, that external actors' direct interventions can be highly consequential for African leaders. Certain leaders have been brought down through external interventions. Some have been kept in power or reinstated through military interventions by outsiders. A powerful neighbor like South Africa can affect longevity of particular leaders. We have not explored external interventions and do not believe that they have systematic effects on time in power for our sample as a whole.

Our analysis does throw light on African patterns where both rapid leadership turnover and remarkably durable leaders can be found side by side. Personal rule requires the development of political networks and power bases. Hence leaders are extremely vulnerable at the outset of their rule, but they become more secure with the passage of time.

Appendix

See the sources cited in the text for a full discussion of hazard models. A hazard model can be described briefly in the following manner. *Duration* is defined as the time in years from entry into power and is broken down into K intervals (for example, 0–2 years, 2–4 years, etc.). We define h_{ik} as leader i's risk of losing power in period k. In the simplest model it is assumed that the risk of losing power is constant for leaders with the same vector of characteristics X_i. Thus in the so called constant rate model,

$$\begin{aligned} Ln(h_i) &= a + X_i'\beta \\ &= a + b_1x_1 + b_2b_2 + \ldots b_nx_n. \end{aligned}$$

In this model a is some constant that effectively captures the underlying risk of losing power. The vector of covariates X_i shifts this risk up or down by some constant multiplicative factor.

In the second model described in the text, the so-called proportional hazards model, a is allowed to change values in each of the K periods defined:

$$\begin{aligned} Ln(h_{ik}) &= a_k + X_i'\beta \\ &= a_k + b_1x_1 + b_2x_2 + \ldots b_nx_n. \end{aligned}$$

The risk of losing power in period k is $e^{ak}e^{Xi'b}$, with e^{ak} as an underlying risk that is time-specific and different in each of the K periods. The model is said to be proportional in the sense that individual i's characteristics are assumed to have the same effect in each period, although the overall estimated hazard may be different because of pure time effects.

Finally, in a third type of model we assume that the effects of covariates vary in their effect on the risk of losing power:

$$Ln(h_{ik}) = a_k + Y_i'\beta_k,$$

where the duration subscript on the coefficient indicates that the effect of the variable changes in each period, even if the variable remains the same. Note that we could posit a model in which the variable changes value over time, but its effect remains the same. We would indicate this by putting the subscript on the covariate and not on the coefficient. Or we could posit that variables vary over time and that their effect are time-dependent as well, and we could put subscripts on variables and coefficient. For example, we tested the hypothesis that age—obviously a time-varying variable—had a time-dependent effect on the risk of losing power.

A general model could include all these assumptions, including constant and time-varying variables with time-dependent and time-independent effects on an underlying hazard rate, which would be constant or different in each period.

Given these different equations for the risk of losing power, it is possible to construct likelihood functions and estimate the coefficients with maximum likelihood methods. These are well described by Allison (1984).

Notes

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1. A good introduction is provided by Miller (1981) and Anderson (1980). Hazard models are well described by Allison (1984). Trussell et al. (1983) provide a useful discussion with examples drawn from demographic data. The reader is referred to these sources for theoretical and computational details. The technical Appendix provides a brief description of hazard models. The multivariate hazard models in this section were all estimated with the software package Rate, which is described in Tuma 1981.

2. We are indebted to Anne Pebley of the Princeton University Office of Population Research for this formulation.

3. For studies in the African context that do focus on coups and stability see Jackman et al. 1986, Johnson, Slater, and McGowan 1984, O'Kane 1981, and McGowan and Johnson 1984. Hughes and May (1988) examine political succession and look at economic and social correlates. They analyze political duration cursorily.

4. The log-likelihood statistic is based on a chi-squared distribution with one degree of freedom and tests the null hypothesis that the two groups face the same hazard distribution. See SAS Institute 1985 (pp. 540–42) for computational details.

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Henry Bienen is James S. McDonnell Distinguished University Professor of Politics and Nicolas van de Walle is a Ph.D. candidate, Woodrow Wilson School, Princeton University, Princeton, NJ 08540.

MODELING THE 1973 SOVIET DECISION TO SUPPORT EGYPT

PHILIP D. STEWART
MARGARET G. HERMANN
CHARLES F. HERMANN
Ohio State University

We present a contingency model of Soviet foreign policy making that focuses on decision making in the Politburo. The model is designed around three questions and shows how the answers to these questions determine the likely nature of the decision the Politburo will reach at any point in time. The questions are (1) Whose positions on the Politburo are critical to making a decision? (2) What are the positions or preferences of those who count on the issue under consideration? (3) How are disagreements among these individuals handled? The model is illustrated by examining the Soviet decision to increase significantly the numbers and types of weapons delivered to Egypt in early 1973. Of interest in this case is accounting for the shift in Soviet policy from refusing Egypt offensive weapons to providing them.

Considerable effort has been devoted to understanding, explaining, and even predicting Soviet foreign policy decisions and behavior. As in the story of the seven blind men and the elephant, however, students of Soviet foreign policy making have tended to focus on only one explanatory factor or a limited set of explanatory factors while believing that they have described the most essential parts of the entire animal. In a thorough review of the literature on Soviet foreign policy making, Horelick, Johnson, and Steinbruner (1975, 53) concluded that no present model provides "comprehensive and consistent explanations and reliable and accurate predictions." Looking to the future, Horelick and his associates proposed that our efforts at modeling Soviet foreign policy making might benefit from the development of frameworks incorporating "multiple perspectives." Writing a decade later, Alexander (1984) still found that models of Soviet decision making tended to emphasize only one set of explanatory factors. As a first step toward more comprehensive models, he suggested a conceptual approach focusing on the relationships among three variable clusters: (1) leaders' perceptions of the external environment (the nation's resources, its technology, and reactions to previous decisions), (2) leaders' beliefs and attitudes, and (3) organizational factors.

We build on Alexander's proposal and develop a contingency model of Soviet foreign policy making that attempts to integrate the components of the decision-making process that students of Soviet politics have identified as important. The model focuses on decision making in the Politburo and takes into account (1) the distribution of power among members of this ruling group as well as (2) the attitudes, perceptions, personal styles, and organizational interests of individual members and (3) the processes by which collective decisions are made.

We will illustrate how the model works

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by examining the Soviet decision to increase significantly the number and types of weapons delivered to Egypt in early 1973. This case is a particularly useful example for several reasons. First, the decision had far-reaching consequences. Not only did it lead to a war that fundamentally altered the position of the Soviet Union in the Middle East, but many believe it also seriously undermined the Soviet policy of détente with the United States. Second, more evidence is available on this decision than on most Soviet foreign policy decisions. Third, a variety of alternative interpretations have been put forth to explain what the Soviet leadership did. Finally, each of these interpretations offers, at best, only a partial explanation. The decision remains a puzzle. Why did the Soviets suddenly do an about-face, first refusing offensive weapons to Egypt and then providing them? Before turning to the model, we describe the problem facing the Soviet Union in the summer and fall of 1972—the months leading up to the decision.

Decision To Supply Offensive Weapons to Egypt

In the late summer and early fall of 1972, the Soviet leadership faced a dilemma in the Middle East. By giving Egypt the strategic, offensive weapons it requested, the Soviets risked a new Arab-Israeli war and being dragged into a superpower confrontation. By not giving the weapons to Egypt, they faced the prospect of losing their position in Egypt permanently.

A prominent Soviet policy objective during this time period was détente with the United States. Détente was perceived as enabling the Soviet Union to obtain Western technology and trade credits, to help limit the arms race, to prevent a military confrontation with the United States, and to counter the risks associated with the U.S.-Chinese rapprochement (see Golan 1984). But to at least some within the Soviet leadership, détente did not extend to superpower relations with Third-World countries. Increasingly in the early 1970s, Marshall Grechko, representing Soviet military interests, pushed for more Soviet involvement in the Middle East, particularly in Egypt (see Golan 1984; Kass 1978; Spechler 1986).

With the succession of Sadat to power in Egypt following Nasser's death in the fall of 1970, Soviet-Egyptian relations became more tenuous. Sadat's purges of pro-Soviet leaders and his approaches toward the West left the Soviet leaders in an uneasy position with an increased concern that "the U.S. and Egypt might work out a Middle East arrangement contrary to Soviet interests" (Freedman 1978, 102). To bolster their position, the Soviet leadership signed a Treaty of Friendship with Egypt and sent Sadat some sophisticated weapons but kept these weapons under Soviet operational control. The Soviet leaders continued to drag their feet on supplying Sadat with the strategic, offensive weapons he thought he needed to wage war with Israel (see Spechler 1986): "The gap between Soviet pledges and Soviet deliveries actually widened during this period" (Breslauer 1983, 91).

Frustrated with Soviet reluctance to make it possible for Egypt to regain its occupied territories and under increasing pressure at home to engage Israel militarily, Sadat ordered the Soviet military out of Egypt in July 1972 and turned toward the West for help. When he did not receive from Western Europe the aid he perceived was possible and found himself being held in abeyance by U.S. leaders, Sadat was forced to turn again to the Soviet Union for military weaponry (see George 1983). These overtures were initiated in the fall and early winter of 1972. This time, as Spechler (1986, 438) observes, "the Soviets promised—and shortly thereafter began to deliver—the amounts of ammunition and parts and types of weapons and equipment that the Arabs considered necessary for going to war." Moreover, they put the weapons under Egyptian control. The Soviet policy toward Egypt changed dramatically.

Scholars have provided a variety of explanations for why the Soviet reluctance and restraint in supplying strategic offensive weapons to Egypt in 1971–72 turned to a willingness to provide these weapons in the winter of 1973. Several of these explanations presume the Soviet leadership acted as a single unit making "rational" calculations about its role in the Middle East based on its relations with the United States. For example, Freedman (1978) explains Soviet behavior as reflecting a real preference for Third-World activism and an underlying lack of serious interest in détente. Breslauer (1983) argues that the Soviet decision to arm Egypt and make war possible resulted from disappointment in the lack of reciprocal restraint on the part of the United States with Israel and U.S. unwillingness to pressure Israel for a peace settlement. This decision was reinforced by low Soviet expectations from détente by late 1972.

A slightly different interpretation suggests the Soviets did not believe their Third-World actions would affect détente. Citing Soviet "misperceptions" of the consequences for U.S.-Soviet détente of the decision to arm Egypt, Glassman (1975, 118–19) sees a continuing strong Soviet commitment to détente throughout this period. Indeed, he indicates that an Egyptian willingness by late 1972 to limit its war aims was critical to the Soviet decision to provide strategic offensive weapons to Sadat (p. 98).

A few scholars (Dawisha 1979; Kass 1978; Spechler 1986) have emphasized the coalition shifts within the leadership in explaining the Soviet decision to support Egypt's move toward a war footing. As Dawisha (1979, 150) notes, the "continuous coalition-building and consensus formation" among competing institutional and leadership groups led to the seeming contradiction in positions toward arming Egypt evident from 1971 through 1973. Focusing on what characterized the competing leadership coalitions, Spechler (1986) stresses the importance of images of détente with the United States. She argues that Soviet decision making on the Middle East during the early 1970s was the product of a struggle among leaders holding "cooperative," "competitive," and "antagonistic" images of East-West relations.

The present model is in this latter tradition. It focuses on the Soviet leaders in the Politburo and seeks to offer a structured and explicit framework for understanding the bases on which coalitions form among members of this decision-making group, the reasons why coalitions change across time, and the process by which consensus is reached. A regime's foreign policy actions—or the lack thereof—result from the interplay of multiple factors, each of which may vary independently of the others. Our purpose is to construct a model of Soviet foreign policy making that suggests how three different factors (*regime, individual qualities, and group dynamics*) relate to one another in accounting for policy in a manner that can accommodate change in any of the contributing factors.

Why should this kind of model be preferable to one designed around one component, for example, a rational actor approach or a focus on national interests? If a regime consists of multiple individuals with different values and, consequently, different preference orderings, the rational actor approach cannot help us reconcile the competing options—each of which may be "rational" from the perspective of one actor. Or if national interests are only one of the sources of explanation for foreign policy—and they do not change over time, but other factors do—how can we calculate whether policy will vary?

In the proposed model each component performs a separate task. The state of each component depends on the nature of the other components. Thus the political regime—or the structure of power and the norms governing its use—determines which persons are going to be influential in a decision. One regime may be a dictatorship with one person's preferences dominating across major issues. In another regime there may be collective leadership. Once the regime component has determined the salient individuals, the model's individual component estimates preferences and the intensity with which each of these individuals holds views on a particular issue based on their personal characteristics, attitudinal data, and organizational affiliations. Individuals vary in the intensity of their preferences on an issue. This variation interrelates with their personal qualities and background to indicate the role they are likely to play in the decision process. Finally, with knowledge about regime norms and roles and preferences, the model estimates how any policy differences among the participants might be handled in terms of group dynamics. With differences resolved, deferred, or deadlocked, we can determine the probable nature of the resulting policy behavior.

In effect, the resulting policy is contingent on each of the components. If the regime changes, the relevant set of actors and the rules guiding their interaction change. Or if certain individuals are sensitive to changes in the relevant environment and dramatic changes occur in that environment, their preferences and roles may change—and so on. Each component can be viewed as providing the answer to a key question, and the resulting policy is contingent on the answers to all three questions. The questions central to each component are, Which leaders are so powerful that their positions must be taken into account in any decision (*regime*)? What factors shape an individual's position and how (*individual*)? How are any differences among key members resolved to determine a Politburo decision (*group*)? In what follows we explicate each component in turn. Before moving to the next component, we describe the procedures we have used for operationalizing key variables in that component and present the data for examining the decision on supplying offensive weapons to Egypt.

The Regime Component: Determining Whose Positions Count

Nature of the Political Regime

Political regimes, defined as a given structure of power and a set of more or less accepted decision norms, have far-reaching effects on the character of the decision process. Not only does a particular regime indicate who can participate in decision making and with what degree of authority, but the structure of a regime determines in important ways the extent to which leaders' personalities and attitudes will impact on the decision.

Thus a first requirement for a model of Soviet foreign policy decision making is to describe the characteristics of the political regime existing within the Soviet leadership at a given time. In our illustrative case, the particular Soviet political regime of interest is that existing in the period 1972 to the middle of 1973. Fortunately for our purposes, Roeder (1984) has proposed a conceptualization of power structures or regimes that builds on what most scholars (e.g., Conquest 1961; Fainsod 1963; Hammer 1974; Hough and Fainsod 1979; Medvedev 1986; Rigby 1970; Rush 1958; Tatu 1969) argue are the dominant features of Soviet politics. The system of top-level leadership in the Soviet Union is characterized by two

Table 1. Soviet Political Regime Types

Structure of Political Authority	Level of Competition for Power		
	Low	Medium	High
Dispersed authority (many)	accommodative bureaucratic (1973–82)	competitive bureaucratic (1983–85)	combative bureaucratic
Clustered authority (some)	ruling coalition	oligarchic (1964–73)	pluralistic (1953–57)
Consolidated authority (one)	dictatorship (1957–59)	directive (1960–62)	first among equals (1962–64, 1985–?)

Source: Adapted from Roeder 1984.

somewhat contradictory traits: tendencies toward authoritarianism or concentration of decision-making authority in one or a few persons; and power struggles or competition for power and authority among the political elite, most notably among those at the Politburo level. These two variables—the structure of political authority and the level of competition for power—together can differentiate in meaningful ways the type of political regime existing in the Soviet Union at any given time.[1] As shown in Table 1, we devise nine regime types by establishing three points on each of these dimensions. Of the nine possible types of political regimes, we can identify seven distinct kinds of political regimes that have existed in the Soviet Union since 1953. Table 1 provides labels for all regime types and shows which have occurred in the Soviet Union.

We have developed a decision tree that asks a series of questions about the structure of political authority and the level of competition in a regime to determine which of these nine types of political regimes exists at any given time and in turn whose positions count in the making of foreign policy. (This decision tree can be found in Stewart, Hermann, and Hermann 1986).

The Soviet Political Regime, 1972 to mid-1973

Using this typology of regimes to determine whose positions count, we need to decide which kind of regime existed in the Soviet Union in 1972–73 at the time of the decision concerning Egypt. A critical factor that distinguishes regimes involving collective authority among multiple leaders from those in which authority is abrogated to a single leader is whether or not a single leader is able to assume the power to make decisions across a range of issues despite varying degrees of opposition. Most studies characterize Stalin's rule between roughly 1934 and 1953 as a dictatorship (for the most influential work, see Fainsod 1963; Hough and Fainsod 1979) and some research sees Khrushchev as having behaved in this manner for much of the last seven years of his rule (e.g., see Hodnett 1981, esp. 88–92). The dominant view in the literature, however, is that Brezhnev never attained this level of authority. Gelman (1984), for instance, characterizes Brezhnev's principle strategy of rule as deliberately seeking to build and stay within the "Politburo consensus." Writing about the early 1970s, Hammer (1974, 319) describes Brezhnev as "the presiding officer of an oligarchy." And Simes (1984, 86) has observed that "during

Brezhnev's political predominance, a consensus style of leadership was clearly evident in the USSR." (See also Hodnett 1981; Rigby 1970.)

If Brezhnev was not able to make decisions on his own, then, was there during this period a relatively small group (i.e., roughly two to five individuals) in the leadership who, if they agreed, could make decisions across a range of issues? In other words, how dispersed was authority among Politburo members? Studies of both protocol evidence (Hammer 1974, 323–24) and political authority (Garthoff 1975; Gelman 1984) in the Brezhnev regime suggest that the inner leadership group in the Politburo toward the end of 1972 consisted of Leonid Brezhnev, general secretary of the party; Alexei Kosygin, prime minister; Nikolai Podgorny, the formal president; and Mikhail Suslov, the powerful, senior ideologue.[2] All were full Politburo members. None owed their position to any of the others. Each had—although in varying degrees—institutional and political bases of support not fully under the control of any other member of the group. Normally when this group found it possible to agree among themselves on policy issues, they could determine policy outcomes.

We do not want to suggest that there was no competition within this inner group, or that they always agreed on outcomes. In fact, Brezhnev's efforts to isolate, if not eliminate, Podgorny's and Kosygin's roles in foreign policy are well documented. (See, for example, Hough and Fainsod 1979, 477–78; Kissinger 1979, 1145). Our point here is simply that at the time of the decision under study it would not be possible to reach and sustain a policy line in the Politburo without the support of these four leaders. None of these core Soviet leaders was sufficiently strong to act alone or could easily or for long act in ways contrary to the interests of the clients whose support contributed to his power base (see Gelman 1984, 51–104). In effect, there was a clustering of authority in these four leaders (row 2 of Table 1).

Could this inner Politburo act when there was resistance to their positions among other members in the leadership? That is, what is the level of competition for power among the members? Which column in Table 1 is appropriate? If it could act despite resistance, the Brezhnev regime in 1972 should be considered a "ruling coalition." The evidence, however, suggests that at least Marshal Grechko, minister of defense; Peter Shelest, first secretary of the Ukraine; and Alexander Shelepin, head of the Soviet Trade Union organization, did on several occasions during this period actively resist policies supported by members of the inner group. Though Brezhnev had succeeded in significantly reducing the institutional power bases of both Shelest and Shelepin prior to the end of 1972 (Gelman 1984, 123–24), these leaders still retained their seats in the Politburo, from which they could make their views heard. Marshal Grechko was a special case. As Gelman (1984, 79–83) documents, Brezhnev decided as early as 1965 to base his own political coalition on an intimate alliance with the military leadership, expressed in part through the all-service military buildup initiated in 1965. Grechko was a vocal and vigorous advocate of both a strong defense and an assertive role for military power in the advancement of Soviet global interests. But even though Grechko was a close ally of Brezhnev at the end of 1972, we do not consider him a part of the inner leadership at that time because he would not become a full member of the Politburo until April 1973. Yet his special political relationship with Brezhnev and his powerful institutional base and role as almost sole spokesman for military interests provided Grechko with a substantial power base from which to challenge the inner leadership's preferences when he disagreed.

If at least some members of the Politburo are able to challenge the inner leadership's decisions without direct penalty and do so regularly, we would label this a pluralistic regime. To the degree, however, that such challenges are met with retaliation, the inner leadership is able to limit effective participation and thus sustain an oligarchic regime. The cases of Shelepin and Shelest in foreign policy; of Gennady Voronov, the moderate agricultural reformer; and Dimitri Polyansky, a reputed Russian nationalist and hardliner on détente, are instructive here. All opposed the Politburo leadership during this period, and all were removed from the Politburo between 1971 and 1975. As Simes (1984, 80) notes, "It appears that those who were expelled from the Politburo had at least one thing in common—a positive program, a desire to advocate relatively strong views, to pressure for some form of change."

Our analysis, then, so far suggests that the Soviet political regime in late 1972 is probably best described as oligarchic (a regime characterized by clustered authority and a medium level of competition for power). In most cases, particularly in foreign policy, the key decision makers were Brezhnev, Kosygin, Podgorny, and Suslov. In certain substantive areas, however, other leaders may well have participated as influential decision makers. The extensive case study literature on Soviet decision making in the October 1973 Middle East war indicates that both Marshal Grechko and Boris Ponomarev, the candidate Politburo member with principal responsibility for Soviet Third-World policy, were active participants in the Soviet decisions relating to these events. (See, in particular, Golan 1984, 189; Kass 1978, 217–23; Shevchenko 1985; Spechler 1986, 453–54.) The most systematic evidence for this position is provided by the frequency of their participation in meetings with Egyptian leaders during the period September 1970–February 1973, either in Moscow or in Cairo. The two most heavily involved actors, aside from the inner leadership, were Grechko and Ponomarev with seven and four meetings, respectively.

Although the analysis that follows develops a probable decision scenario limited to these six actors, we do not want to suggest that no other leaders' views may have been taken into account. Rather, our argument is that given the prevailing structure of oligarchic authority in the Politburo at this point in time, others' views probably influenced a specific policy choice only to the extent that they became an important source for the judgments of one of the six identified decision makers.

The Individual Component: Assessing the Positions of Those Who Count

Factors Shaping Individual Positions

Once we know which Politburo members are important in determining Soviet policy vis-à-vis Egypt during the 1972 to mid-1973 period, we need to determine the nature of their positions or preferences on the critical issue of interest in the case, namely, whether or not to enable Sadat to go to war against Israel by supplying his country with strategic, offensive weapons. Do the members whose positions count favor or oppose arming Egypt and how strongly do they hold their opinions? Literature on Soviet leaders (e.g., Alexander 1984; Bialer and Afferica 1986; Breslauer 1982; Herrmann 1985; Roeder 1985; Stewart 1986; Valenta and Potter 1984), more general literature on foreign policy making (e.g., Brecher 1975; East, Salmore, and Hermann 1978; George 1980; Hermann, Kegley, and Rosenau 1987; Jervis 1976; Steinbruner 1974; Sylvan and Chan 1984; White 1970), and research on social and political cognition (e.g., Ajzen and Fishbein 1980; Fazio 1986; Lau and Sears 1986; McGuire 1985; Petty and Cacioppo 1986) suggest four factors are among the most important potential influences on a member's position. These factors include perceptions of the international environment, personal attitudes on important issues, personality characteristics, and organizational background and affiliation. The studies cited above indicate that these factors combine the more transitory and stable influences on the individual choice-making process, that they can be measured, and that they affect not only how decisions are made but the nature of the decisions.

Before indicating how these four factors might combine to shape a Politburo member's position on an issue, we will indicate what is meant by each of the factors, describe what aspects of each we must assess in determining a member's choice with regard to the issue of rearming Egypt, and discuss our assessments of these four factors in the present study.[3] Table 2 presents empirical data for the four factors for the six members of the Politburo who were involved in making the decision to supply offensive weapons to Egypt. These data are drawn from M. Hermann's (1980b) and Stewart's (Stewart, Warhola, and Blough 1984) content analyses of the speeches and writings of these Politburo members from January 1970 through December 1972.[4]

Table 2. Factors Affecting the Positions of the Politburo Members Whose Positions Counted

Politburo Member	Salience of Issue[a]	Commitment to Preference[b]	Sensitivity to Contextual Information[c]	Relation to Organizational Sector[d]
Brezhnev	.01 (h)	.16 (w)	65 (h)	generalist
Kosygin	.03 (h)	.22 (w)	68 (h)	generalist
Podgorny	.00 (l)	.16 (w)	97 (h)	generalist
Suslov	.00 (l)	.18 (w)	35 (l)	careerist (party ideologue)
Grechko	.01 (h)	.44 (s)	69 (h)	careerist (military)
Ponomarev	.00 (l)	.25 (w)	58 (h)	careerist (party ideologue)

[a] Numbers are percentages of total references to foreign policy issues that focused on the Middle East. The letters in parentheses indicate whether these percentages suggest high (h) or low (l) salience of this issue to the Politburo member based on the mean of all the members of the Politburo. The mean across all members was .01.

[b] Numbers are percentages of references to issues supportive of rearming of Egypt compared to references both supportive and resistive to rearmament. The letters in parentheses indicate whether these percentages suggest a weak (w) or strong (s) commitment to a preference. A member's score was considered to indicate strong commitment if it was one standard deviation or more above the overall Politburo mean of .24; one standard deviation was .14. Otherwise the score was considered indicative of weak commitment.

[c] The numbers are standard scores based on a mean of 50 and a standard deviation of 10 indicating how responsive the Politburo member is to the political environment in speech material. The letters in parentheses indicate whether the standard score is one standard deviation above (h) or below (l) the mean.

[d] A generalist is a Politburo member who gained his position by moving across institutions and organizations whereas a careerist has generally come up through one organization or institution. Careerists have a strong affiliation, generalists a weak affiliation with the organizational sector.

Perceptions of the international environment. Brecher (1975) has observed that how leaders perceive or interpret the international environment provides us with cues as to the choices they will want their governments to pursue. Examining the writings and statements of Politburo members, Stewart and his associates (Stewart, Blough, and Warhola 1984; Stewart, Warhola, and Blough 1984) have shown that the issues and areas of the world emphasized by a Politburo member reflect perceived priorities—how salient the issue or area is to the member. In other words, perceptions of the international environment suggest how the member structures the world and what problems are seen as important.

In looking at the Politburo decision to supply Egypt with strategic, offensive weapons, we are interested in how salient Middle East issues were perceived to be to each of the members, that is, how much they discussed the Middle East and its problems or opportunities relative to other topics and issues. Salience not only indicates what is important to the member but suggests whether the member will have a position on the issue. Members are less likely to have thought through or defined a position on an issue that is of little salience to them.

The first column of Table 2 presents data for perceived salience of Middle East issues for the six members of the Politburo whose positions were important to the decision process. Perceived salience was assessed by ascertaining the percentage of all references to foreign policy the Politburo member made from January 1970 through December 1972 that focused on Middle East politics. Such references took the form of comments about the Palestinians, U.S. actions in the Middle East, Soviet involvement in the region, Israeli actions, Middle East peace efforts, and the policies and behavior of the Arab countries. The Middle East was considered a salient issue for a member whose score was at or above the mean for the whole Politburo on this issue.

Attitudes. Attitudes represent particular ways of viewing the world that are evaluative in nature. Herrmann (1985) and Stewart (Stewart, Warhola, and Blough 1982) have shown how the Politburo members' attitudes shape their orientations to foreign affairs. By knowing a member's more general international attitudes, we can infer a position on a particular issue in the absence of more specific information about the member's views. In considering Politburo members' attitudes toward providing Egypt with offensive weapons, we will examine members' reactions to a set of issues we regard as indicative of either support for or resistance to supplying such weapons to Egypt. The supportive issues focus on the importance of advancing Soviet interests in the Third World. The resistive issues indicate the usefulness of détente and constructive interaction with the West.

The particular issues we have chosen to examine build on the cooperative and antagonistic images of the United States that Spechler (1986) identified among Politburo members during the 1967–73 time period. Spechler observed that these two images had implications for the Arab-Israeli conflict. Those members holding the cooperative image "were impressed by the fragility of détente. . . . They did not want events in the Middle East (or in any other region) to cause or deepen mistrust and hostility between the U.S. and the U.S.S.R." (Spechler 1986, 448). According to those with the antagonistic view, "the U.S. must be actively countered and restrained, both militarily and politically. Highest priority must therefore be given to expansion of Soviet military power and political influence on a global scale. The U.S.S.R. must acquire friends and military facilities and maintain loyal allies wherever these would help to combat, contain, or undermine American power" (Spechler 1986, 450).

The second column of Table 2 displays data on attitudes in the form of commitment to preference. The direction of a member's position toward arming Egypt (for or against) and the degree of commitment to this position were determined by finding the percentage of references a Politburo member made that were supportive of rearming Egypt compared to that member's total references both supportive and resistive to rearmament. Following Spechler's (1986) analysis, references were considered supportive of supplying Egypt with offensive, strategic weapons if they focused on the importance of Soviet support for national liberation movements and involvement of the Soviet Union in the Third World contained an emphasis on the dangers of détente and viewed the West as an irreconcilable enemy. References were classified as resistive if they saw Western behavior as moderate and the West as governed by "sober-minded forces" as well as showed an interest in arms limitation and disarmament, Middle East peace efforts, and fears of the increasing risks of nuclear war. The higher the member's score on commitment to a preference, the more supportive the member was considered to be of arming Egypt, and the lower the commitment score, the more resistive the member to making it possible for Sadat to attack Israel. A Politburo member was viewed as committed to a position if the score was one standard deviation above or below the commitment mean for the Politburo as a whole.

Personal characteristics. A Politburo member's personal characteristics also provide us with information about positions on policy questions. In particular, M. Hermann (1980a, 1980b, 1984, 1987) has shown that sensitivity to contextual information has an effect on how Politburo members and other heads of state around the world process information and whether outside forces can influence their decisions.

Sensitivity to contextual information indicates how open a member is likely to be to information from the environment in determining the position to take on an issue facing the Politburo. The more sensitive to contextual information a member is, the more receptive he is to cues from the environment—information about the political context at the moment, information about other members' positions, organizational views—as guides to decision making. The less sensitive the member is, the more likely the member is to impose a previously established and strongly held set of views on the environment, selectively perceiving information that supports or bolsters the member's prior framework. Sensitivity to contextual information, in effect, separates leaders who are pragmatists from those who are crusaders (see Stoessinger 1979).

Data on sensitivity to contextual information is reported in column 3 of Table 2. To assess sensitivity to contextual information, a member's speeches and writings were content-analyzed for indicators of conceptual complexity. Conceptual complexity refers to the degree of differentiation that an individual shows in describing or discussing other people, places, policies, ideas, or things. The more conceptually complex individual can see varying reasons for a particular position, is willing to entertain the possibility that there is ambiguity in the environment, and is flexible in reacting to objects or ideas. The score for conceptual complexity is the percentage of words used that indicate high complexity. The coding categories and reliability data for this variable for these Politburo members are available in M. Hermann 1980b, 1983. The numbers for sensitivity to contextual information listed in Table 2 are standard scores based on a mean of 50 and a standard deviation of 10. The standard scores are derived from the mean score and standard deviation for conceptual complexity for all Politburo members during the 1970–72 time period. A member is considered highly sensitive to contextual information whose score is one standard deviation above the mean and rather insensitive whose score is one standard deviation below the mean. Because Ponomarev's score is quite close to one standard deviation above the mean, he was considered more rather than less sensitive to the context in the present study.

Organizational background. The adage "Where you stand depends on where you sit," used to describe U.S. bureaucratic politics also applies in the Soviet case and becomes the last factor important in identifying the position of Politburo members. Soviet leaders have backgrounds in a variety of institutions and in some cases represent these institutions on the Politburo. Those with longer tenures in a particular organization (e.g., the KGB, the foreign ministry, the International Department of the Party Secretariat) can be expected to be imbued with their organization's mission and to see the world through the mission requirements of that organization (see, e.g., Ashley 1980; Axelrod 1976a; C. Hermann 1978; Lowenhardt 1981; Valenta 1979).

Roeder (1986) has proposed two types of participants in the Soviet policy-making process: generalists and careerists. The generalists have gained their position of influence by moving across institutions and organizations—having experience and patrons in several different arenas. The careerists, on the other hand, have come up through one organization or institution—gaining expertise and a strong sense of identify with that organization. The careerists tend to view the world in ways that justify and strengthen the goals of the organizations with which they have been affiliated. In effect, the generalist's political socialization produces a more heterogeneous perspective on priorities and operations in politics and world affairs, whereas the careerist's political socialization more likely has provided a singular perspective. Thus the mission of the organization with which a careerist is affiliated provides us with a means of knowing this member's position. It will be more difficult to ascertain the generalists' positions from their work experience.

Strength of organizational affiliation was based on a biographical search of each member's career patterns (Bishop 1986; M. Hermann 1980b). A member who had held positions for a 5–10 year period of time in more than one type of organization or institution or had lengthy service in a position with broad responsibilities across a number of issue areas was considered a generalist. A member was judged to be a careerist if most (roughly 75%) of his party or government experience was in one type of organization or institution, for example, the KGB, the foreign ministry, or the defense industrial establishment.

How Factors Combine To Shape an Individual's Position

To help us understand how a Politburo member's perceptions, attitudes, personal characteristics, and organizational background interrelate in determining a position on a specific issue, we have derived a set of decision rules from the research literature on information processing (e.g., Axelrod 1976b; Fiske and Taylor 1984; Frey 1986; George 1980; Jonsson 1982; Lau and Sears 1986; Schank and Abelson 1977; Simon 1983; Steinbruner 1974; White 1984; Wicklund and Brehm 1976).[5] These decision rules indicate that Politburo members become *advocates, cue takers,* or *brokers* on a specific issue depending on how salient the issue is for them and the level of their commitment to a position. How they elect to play these roles in the decision-making process is affected by their sensitivity to contextual information and the strength of their organizational affiliation. Figure 1 presents a decision tree that shows how the factors combine to shape a member's position. Using the data in Table 2 and Figure 1, we can estimate the role probably taken in the decision-making process by each of the members whose positions counted in the decision to rearm Egypt.

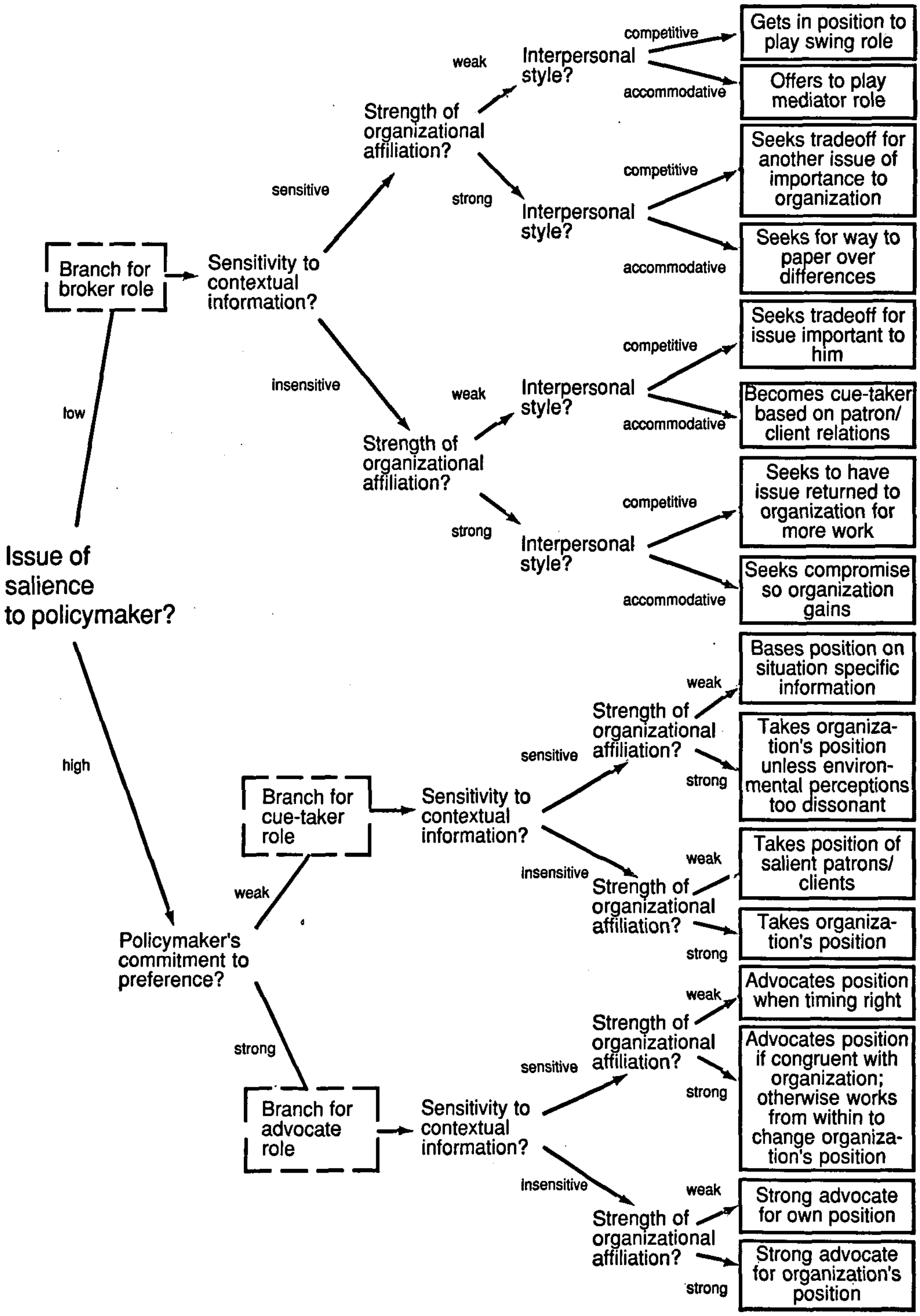
Advocate. The basic decision rule for becoming an advocate in the decision-making process can be stated as follows: If the issue is both salient to a Politburo member and he has expressed a strong opinion on it—either pro or con—the member is likely to look for information in the environment that supports the position and to reinterpret any organizational mission statement to encompass this view. In other words, the member will look for consonant, not dissonant, information that bolsters the position and provides a rationale for the option the member supports. Because the issue has personal salience, the member's expressed attitude will take precedence over perceptual or organizational factors. This set of relationships will be particularly potent for the individual who is relatively insensitive to contextual information anyway—who has a well-developed lens to view the world through. In this case, the member is likely to rationalize the information to support the position. However, the Politburo member who is fairly sensitive to contextual information will move more cautiously in advancing the position while holding firmly to it. Members who are advocates may act as coalition leaders if there are discrepant views being voiced on the Politburo.

Applying the data from Table 2 to Figure 1, we note that Marshal Grechko is the only one of the six key members in the Egypt decision who was an advocate of a position. The Middle East was a highly salient issue area for him and he was committed to supplying sophisticated weapons to Sadat. The data in Table 2 also indicate that Grechko is sensitive to contextual information and has a strong affiliation with the defense establishment in the Soviet Union. Moving through the decision tree in Figure 1 for members who are advocates, our information on Grechko places him in the outcome category that suggests he will advocate his position if it is congruent with that of the organization with which he is affiliated or, if not, he will work to change the organization's position from within. What was the Defense Ministry's position on Egypt in 1972?

An examination of leading Soviet publications during the 1970–72 time period shows the military press as a chief proponent of an increased Soviet military involvement in the Middle East (see, e.g., Golan 1984; Kass 1978; Spechler 1986). *Krasnaia zvezda*, the official press organ for the Ministry of Defense, argued that the "preservation of the Soviet-Arab alliance was a primary national interest of the USSR" (Kass 1978, 222). As Spechler tells us, "Influential military leaders . . . wanted to give priority to the Soviet-Arab alliance, not to détente, and . . . advocated both a more sympathetic attitude toward Arab use of force and deeper Soviet military involvement in the Arab-Israeli conflict" (1986, 452). Thus, Grechko's position was isomorphic with that of his institutional base and he became an advocate for the position in the Politburo.

Cue taker. The information-processing literature suggests what happens when the issue is salient to a Politburo member who has not decided on a position or has only a weak commitment to a position. This individual will become a cue taker, making decisions on the basis of what others are doing or what is happening in the immediate situation. The issue is important

Figure 1. Conditions under Which Member Acts As an Advocate, Cue Taker, or Broker


Issue of salience to policymaker?
low
Branch for broker role
Sensitivity to contextual information?
sensitive
Strength of organizational affiliation?
weak
Interpersonal style?
competitive
Gets in position to play swing role
accommodative
Offers to play mediator role
strong
Interpersonal style?
competitive
Seeks tradeoff for another issue of importance to organization
accommodative
Seeks for way to paper over differences
insensitive
Strength of organizational affiliation?
weak
Interpersonal style?
competitive
Seeks tradeoff for issue important to him
accommodative
Becomes cue-taker based on patron/client relations
strong
Interpersonal style?
competitive
Seeks to have issue returned to organization for more work
accommodative
Seeks compromise so organization gains
high
Policymaker's commitment to preference?
weak
Branch for cue-taker role
Sensitivity to contextual information?
sensitive
Strength of organizational affiliation?
weak
Bases position on situation specific information
strong
Takes organization's position unless environmental perceptions too dissonant
insensitive
Strength of organizational affiliation?
weak
Takes position of salient patrons/clients
strong
Takes organization's position
strong
Branch for advocate role
Sensitivity to contextual information?
sensitive
Strength of organizational affiliation?
weak
Advocates position when timing right
strong
Advocates position if congruent with organization; otherwise works from within to change organization's position
insensitive
Strength of organizational affiliation?
weak
Strong advocate for own position
strong
Strong advocate for organization's position

to these members. The question is how best to deal with it. They rely on the context to answer this question. Whence they take their cues depends on how sensitive they are to contextual information.

The less sensitive the member to nuances in the environment, the more the member will take recommendations or interpretations from significant others. The member who is strongly affiliated with a particular organizational sector is likely to adopt the position that predominates in that organization. The one who is only weakly affiliated with an organization will probably take cues from patrons or clients if they strongly advocate a position. In effect, the cue taker's power base supplies a position to the cue taker who is more insensitive.

For the Politburo cue taker who is more sensitive to contextual information, perceptions of what is happening currently become important to determining a position. The member who has a strong organizational affiliation is likely to compare the organization's position against personal perceptions of the international environment at that point in time. If these perceptions do not prove too discrepant, that is, if they tend to support the organization's option, the member will adopt the organization's position. But should the dissonant information from the environment lead to another position, the member is likely to adopt a position different from that of organizational comrades. The member with a weak organizational affiliation will depend on situation-specific cues and go with the majority opinion of the moment.

The data in Table 1 suggests that there were two cue takers among the key Politburo members on the issue of rearming Egypt: Brezhnev and Kosygin. For both men the Middle East issue was highly salient. Yet their commitment scores are not one standard deviation above or below the mean. Although Brezhnev and Kosygin leaned toward resisting sending offensive arms to Egypt, their positions were not strong. Applying the data in Table 2 to Figure 1, we note that both these men were sensitive to contextual information and generalists with weak ties to any organization or institution. The decision tree indicates that such cue takers are likely to base their positions on situation-specific information. In effect, in forming their positions Brezhnev and Kosygin are likely to depend on what is happening in the Middle East and elsewhere at the time a decision needs making as well as on the majority opinion among other Politburo members.

Broker. Politburo members for whom the issue under consideration is *not* salient are likely to play a broker role in the decision-making process, helping to resolve the differences among other members while accruing credit for issues of importance to them. In effect, these members have less to lose and can gain as a result of what happens in the course of making a decision. So although they are indifferent on the substance of the issue under consideration, it is to their advantage to play a role in the process of working through conflict among other members of the Politburo.

The information-processing literature suggests that interpersonal style becomes a relevant factor here in addition to sensitivity to contextual information and strength of organizational affiliation because these members become involved in shaping the dynamics of the group as they try to resolve the differences among other members. How they choose to play this role—whether in an accommodative or competitive manner—is influenced by their interpersonal style. Specifically, are they cooperative and able to see situations and people in a win-win way or are they combative, tending to see politics as a zero-sum game?

Politburo members, whether insensitive or sensitive to information, who are strongly affiliated with an organization are likely to play the role of delegates or representatives of their organization when the situation is not personally salient. They want to get the best deal they can for their organization. The sensitive leaders will be able to use the nuances of the situation more than the insensitive leaders. But their aim is the same—to "get what we can for our organization." The more accommodative members playing a broker role will work on compromises or papered-over solutions that let their organizations save face or help them form a coalition with another organization. The more competitive members will try to ensure that their organization gains as a result of the decision even if it is only a quid pro quo for the future.

Politburo members in a position to play a broker role with weak links to an organizational sector focus on increasing their *own* power and influence. Those who are more sensitive to the context are probably more effective in realizing their goal than those who are less sensitive. The less sensitive members are likely to be more obvious in their behavior and thus more easily dealt with or discounted. The more sensitive members, however, will give the appearance of being helpful while gaining influence for the future.

When the data in Table 2 are applied to Figure 1, it becomes apparent that three of the members whose positions counted in the decision to arm Egypt played a broker role: Podgorny, Suslov, and Ponomarev. For all three, salience on the issue was low relative to other Politburo members. These three members differed in how sensitive they were to contextual information and in strength of organizational affiliation. Podgorny was sensitive to contextual information and was not strongly affiliated with any organizational sector. Data on interpersonal style from M. Hermann (1980a, 1980b) indicate that Podgorny was likely to be accommodative.[6] As Gelman (1984, 73) observes, "Some of his colleagues saw Podgorny as lacking steel in his makeup." According to Figure 1, with these characteristics, Podgorny is likely to offer to play a mediator role in this decision process.

Although Ponomarev and Suslov were both strongly affiliated with specific institutional interests—Suslov as the Party ideologue and Ponomarev with the International Department (the department most responsible for Third-World policy), they differed in their sensitivity to contextual information and interpersonal style. Suslov was relatively insensitive to contextual information and accommodative, whereas Ponomarev was moderately sensitive but competitive (or combative) in style. Thus, according to Figure 1, Suslov was likely to seek a compromise consistent with his ideological principles, and Ponomarev was likely to look for a trade-off for another issue of importance to his organization. In the Egyptian case, the trade-off for Ponomarev might be Politburo support for actions to change the Third World nations' image of the Soviet Union as an unreliable ally that arose with Sadat's expulsion of the Soviet military (see Spechler 1986, 453).

Group Component: Resolving Differences among Those Who Count

Politburo Collective Decision Making

In a political regime characterized as a dictatorship and in some circumstances in a directive, or first-among-equals, regime, the preferences of a single individual become the Politburo position on an issue. (For a definition of these types of regimes, see Table 1). In all other circumstances the positions of several powerful individuals must be considered and our model must have a component that takes into account the collective processes by which disagreements among these people are handled.

The task for this final component of the model, then, is to establish an analytic system for moving from individual preferences to a collective decision (or indecision). To develop that system we draw on insights from research on Soviet politics and from studies of group dynamics (e.g., Brandstatter and Schuler 1978; Jones 1984; McGrath 1984; Triska and Finley 1968; Valenta 1979; Valenta and Potter 1984). Our typology of political regimes reveals which members of the Politburo we can expect to be essential for the resolution of a problem. Our assessment of the factors shaping perspectives on the issue indicates whether each individual member will be a broker, cue taker, or advocate (and, in the case of an advocate, the direction and intensity of the position). Using this knowledge, we create a set of decision rules for how those members whose positions count will handle disagreements among themselves. Rules must be developed for each of the nine types of political regimes in Table 1 and for any configuration of advocates, brokers, and cue takers.

Among the key assumptions on which the decision rules rest are the following:

1. *Regime effects on who must concur.* The number of Politburo members who must concur with a position in order for it to be a Politburo decision generally increases with both heightened competition among the members and more diffusion of authority and, conversely, decreases with reduction in competition and consolidation of authority. (We have identified these as the defining characteristics of a political regime.)
2. *Determining the core group.* The type of regime and the nature of the issue determine which Politburo members' support is necessary in order to obtain a Politburo decision. (We call this subset of members the core group.)
3. *Core group consensus.* When the core group of the Politburo share the same position, even in the face of opposition from other members, that core position becomes the Politburo's collective decision.
4. *Decision by the most powerful.* When power and authority are unequally distributed within the core group, those with the most power and authority will seek to resolve any differences on the issue among themselves and exclude other members.
5. *Expansion of influence.* When the core group of the Politburo cannot resolve its differences, other members become more influential in determining the group's decision.
6. *Factors affecting weight of influence.* When the views of other members of the Politburo must be taken into account, those who have greater expertise on the issue, who represent interests directly affected by it, or who are special clients of leading members of the core group will exercise more influence than others.
7. *Changing positions.* Shifts in the position of any Politburo member as a result of developments in the external environment are more likely if the member (a) does not currently hold a position intensely and (b) has the personal characteristics to be receptive to new environmental inputs.
8. *Conditions for deadlock.* As the intensity of disagreement among members increases or as the number of key members who must concur with a position in order for it to become a Politburo decision increases, deadlock becomes more likely.

9. *When brokers succeed.* Brokers within the Politburo are more likely to succeed in creating an acceptable compromise between conflicting positions, the lower the intensity with which members hold opposing views.
10. *Costs of exclusion.* As the competition for power becomes more acute and continuous, the exclusion of the preferred position of any competitors becomes more costly in terms of maintaining the existing regime.

These assumptions provide the foundation from which decision rules have been designed for each regime type. The purpose of the decision rules is to estimate decision outcomes for different configurations of individual stances among key Politburo members. The rules for each type of regime have been organized into a series of decision trees. As noted earlier, we determined that an oligarchic regime prevailed in 1972 when the Politburo struggled with the issue of rearming Egypt. Therefore we will apply the decision tree for oligarchic regimes to this case.[7]

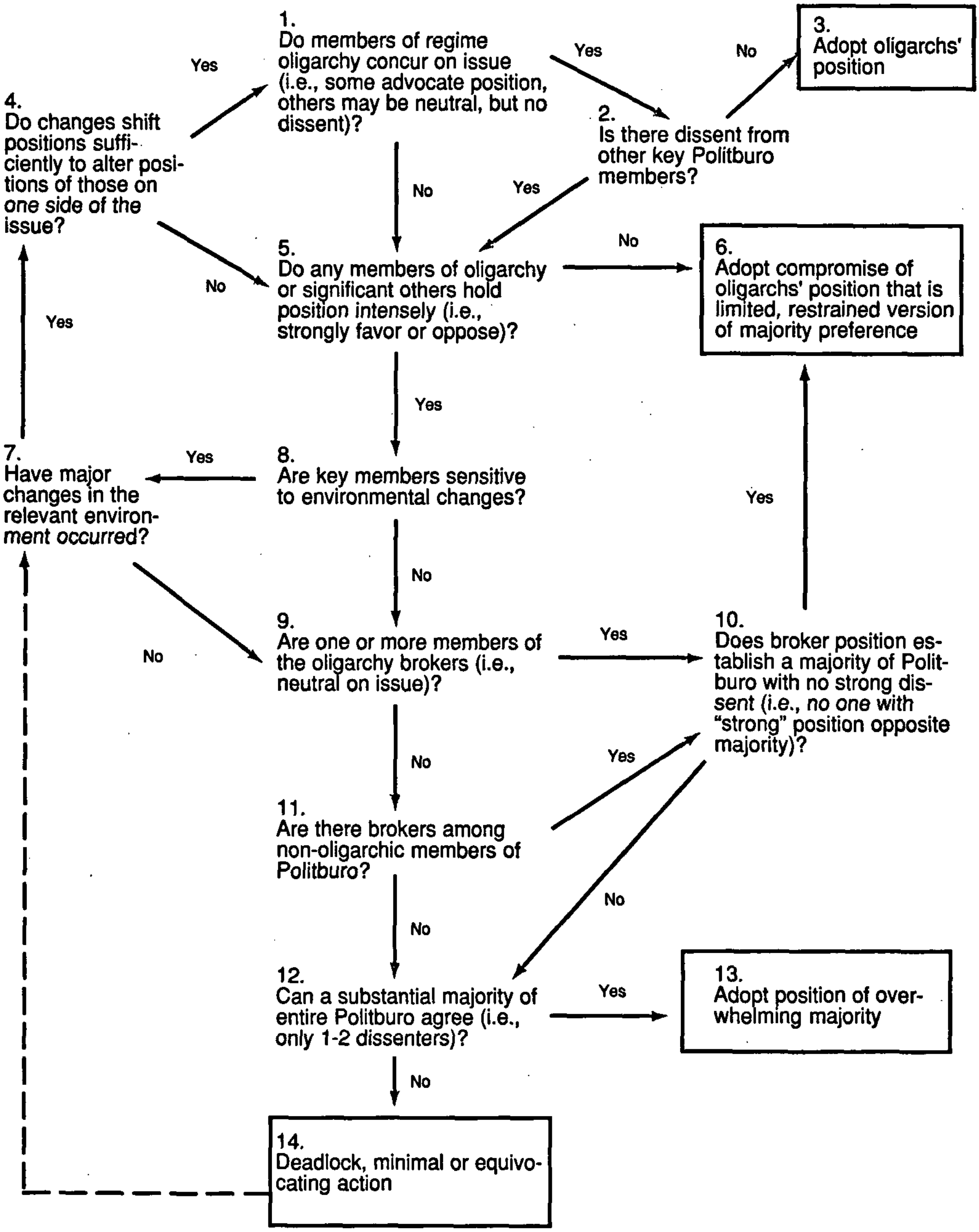
Nature of Collective Decision on Arming Egypt

Let us begin by estimating the situation in the Politburo as it may have appeared in the late spring of 1972. As shown in the decision tree in Figure 2, we start by asking about the possible concurrence of positions among the four leading members of the oligarchy (item 1 in Figure 2). As indicated, our available data suggest Brezhnev and Kosygin were leaning against greater involvement, but they are classified as cue takers. Suslov and Podgorny had been relatively neutral on the issue and are categorized as potential brokers. Even though our data indicate their commitment was modest, there was no disagreement among these four on the position of withholding arms from Egypt. Had these members constituted the core of a ruling coalition regime, the absence of dissent in the inner circle would have been sufficient for us to expect a firm policy decision. But in an oligarchical regime there is more competition for power, and the core group is mindful of the need to consider the views of key clients or others claiming special interest in the problem. Thus we must consider item 2 in Figure 2 and determine if other key members on this issue dissented from the core group's disposition to withhold offensive arms. We have contended that on this issue the positions of Grechko and Ponomarev must be added to the views of the oligarchs. Ponomarev, the data imply, was rather indifferent on the issue, but Grechko emerged as a strong advocate of providing Egypt with substantial armament. Because of his dissent, we have a division among those whose positions count. The model must propose how the issue would be dealt with under these circumstances.

Our assumption is that the core group has a strong desire to resolve issues among themselves—to limit the number of people who must be included in the decision. If no one feels intensely about the issue, then there is the possibility of the core group reaching a compromise among themselves (item 5 in Figure 2 leading to item 6). In this case, however, Grechko held his position quite strongly (more than one standard deviation from the mean). We judge that under these circumstances easy compromises are unlikely.

Some resolution of the problem, however, is still possible if any of the policymakers whose positions count are sensitive to what happens in the environment and modify their positions as a result of major external events (item 8 in Figure 2). When the decision-making group includes such individuals—and we have estab-

Figure 2. Sequence of Decision Rules for Resolving Disagreements in an Oligarchic Regime


1.
Do members of regime oligarchy concur on issue (i.e., some advocate position, others may be neutral, but no dissent)?
Yes
No
2.
Is there dissent from other key Politburo members?
No
Yes
3.
Adopt oligarchs' position
4.
Do changes shift positions sufficiently to alter positions of those on one side of the issue?
Yes
No
5.
Do any members of oligarchy or significant others hold position intensely (i.e., strongly favor or oppose)?
No
Yes
6.
Adopt compromise of oligarchs' position that is limited, restrained version of majority preference
Yes
7.
Have major changes in the relevant environment occurred?
Yes
No
8.
Are key members sensitive to environmental changes?
No
Yes
9.
Are one or more members of the oligarchy brokers (i.e., neutral on issue)?
Yes
No
10.
Does broker position establish a majority of Politburo with no strong dissent (i.e., no one with "strong" position opposite majority)?
Yes
No
11.
Are there brokers among non-oligarchic members of Politburo?
Yes
No
12.
Can a substantial majority of entire Politburo agree (i.e., only 1-2 dissenters)?
Yes
No
13.
Adopt position of overwhelming majority
14.
Deadlock, minimal or equivocating action

lished that both Kosygin and Brezhnev manifested such responsiveness—we must monitor the environment for the possible occurrence of position-shaping events. None pertaining to the Egyptian question appear to have transpired in the spring of 1972. In fact, the events that did occur, such as the summit with Nixon and the Indo-Pakistani War, could be expected to reinforce Kosygin's and Brezhnev's sense of caution about providing weapons to Egypt. These events seem to undermine Grechko's position. Our analysis suggests that he, too, was sensitive to changes in the environment, although he was less likely to shift his position significantly, because of the reinforcing effect of the military interests that he probably felt compelled to represent.

Even though an easy compromise may not have been possible, a bargain among opposing members was likely if some of the core group were indifferent on the issue and were in a position to be brokers or mediators (item 9 in Figure 2). The available data suggest that on this issue Podgorny, Suslov, and Ponomarev all may have been able to play such a role. With half the members who comprise the core group somewhat opposed to sending more arms to Egypt, and Grechko's advocacy for supplying armaments somewhat tempered by external events, the opportunity arose for forging a compromise (item 10 in Figure 2 leading to outcome item 6). Item 6 in Figure 2 summarizes the outcome expected by the model under the circumstances suggested by the data. Specifically the model's forecast is for a restrained version of the majority position (no offensive weapons). Through the spring of 1972 and into the summer, Soviet behavior would appear consistent with this expected outcome. No substantial quantity of arms were transferred to Egypt—just enough to keep them on the string.

In the summer and autumn of 1972, however, Sadat and his regime initiated actions that rightly could be regarded as restructuring events for the Soviet Politburo. In July 1972 Sadat boldly demanded the withdrawal of Soviet military advisors from Egypt. Our model suggests that such action would be perceived and affect the calculus of two pivotal members of the oligarchy—Brezhnev and Kosygin—whose personal profiles indicate that they are sensitive to environmental changes (see item 7 in Figure 2). Sadat's summer action demonstrated that he was prepared to disconnect himself militarily from the Soviet Union, which, in turn, could significantly reduce the USSR's position and influence in the Middle East. In the fall, Sadat took a second major step by indicating to the Soviets that he would limit his war aims—that he was prepared to engage Israel for limited objectives and thus reduce the risk of the war escalating to involve the Soviet Union directly. He also assured the Soviets of naval access to Egyptian ports.

We assume Sadat's two dramatic steps in the summer and autumn of 1972 were significant to at least Brezhnev and Kosygin, making possible a realignment of the positions of the core group of the Politburo. In terms of the decision tree in Figure 2, the answer to question 7 becomes *yes* sometime during the fall of 1972. Two key players who had been opposed to providing arms to Egypt were forced by external events (Sadat's demand for withdrawal) to realize the Soviets could lose their position in the Middle East. Sadat's subsequent actions combined this threat with a bid for Soviet support. The Soviet leadership was given another chance. A consensus is now possible in the core group of the Politburo around the Grechko position (item 3 in Figure 2). If the only members of the Politburo for whom the issue is salient agree on a course of action, that position becomes the decision and the outcome (item 3). Thus, the model now expects a decision to rearm Egypt.

Table 3. Percentage of Statements by Inner Politburo Members Supportive of Factors Associated with Giving Offensive Arms to Egypt

	1st Quarter 1972	2d Quarter 1972	3d Quarter 1972	4th Quarter 1972	1st Quarter 1973
Brezhnev	.20	.18	0[a]	.10	.16
Kosygin	.37	.25	.59	.24	.51
Grechko	.59	.52	.43[a]	.39	1.00[a]
Suslov	.33[a]	.08	.10	.34	.60[a]
Podgorny	0[a]	.08	0[a]	0[a]	0[a]
Ponomarev	.20	.10	0[a]	0[a]	.32
Politburo mean	.16	$.1\overline{0}$	$.0\overline{6}$	.11	$.1\overline{5}$
Standard deviation	.21	.14	.16	.16	.22

[a] Indicates less than 10 statements during the quarter that could be coded as evaluating factors relevant to supporting or opposing providing offensive arms to Egypt.

Do the available data support the model's expectation of a realignment of the positions of the key members of the Politburo most sensitive to major environmental developments? Table 3 displays the percentage of public statements favorable to arming Egypt made by each of the Politburo's inner circle during each quarter of 1972 and the first quarter of 1973. More specifically, entries in the table are references to the various factors that we judged would be supportive of enabling Egypt to go to war as a proportion of all references to factors both supportive and resistive of arming Egypt.

The data for the first two quarters of 1972 reflect the pattern described previously. Grechko appears as a strong advocate of supplying offensive weapons. The others are, to varying degrees, inclined in the other direction. During the third quarter of 1972, Sadat took his initiatives. The model suggests that these events ought to have the greatest impact on Brezhnev and Kosygin, the two cue takers. Kosygin's statements in the third quarter do reflect a rather dramatic change. For the first time, over 50% of his statements (more than two standard deviations above the Politburo mean) emphasize supportive factors. One might argue that Sadat had an effect on Brezhnev as well, but in a different way. The Soviet leader who had been one of the most active public spokesmen simply stopped referring to the relevant factors entirely.[8] So did Podgorny and Ponomarev, although this is not a major change from their previous behavior. Grechko, too, makes very few public statements, but they remain favorable to support for Egypt.

The fourth quarter of 1972 seems more puzzling. Of the four members of the core group who speak out, three (Brezhnev, Kosygin, and even Grechko) place more emphasis on the factors indicative of resisting sending arms to Egypt than during any of the three previous quarters. The exception is Suslov, one of the members we had designated as a possible broker—the member least likely to change his views in response to external developments but likely to be responsive to opportunities within the group. On balance Suslov still stresses the supportive factors less than 50% of the time, though he is speaking publicly in a pattern that during the fourth quarter of 1972 comes closest to the now more cautious position of Grechko.

The data for the first quarter of 1973 should reflect a new Politburo decision on Egypt, and they seem to do so. Grechko and Suslov now say very little publicly, but their few statements strongly emphasize the factors supportive to arming Egypt. Kosygin, the more active spokesman during this period, also stresses the supportive factors. Podgorny continues his practice of saying nothing that provides indicators of his position. Ponomarev still gives limited emphasis to supportive factors, but more so than in any previous quarter. It is Brezhnev, the proponent of détente with the West, who appears to remain unconvinced. His references to the resistive and supportive factors are still tilted against enabling Egypt to engage Israel in war.

Taken together, the data in this case support an interpretation consistent with the model's expectations. Sadat's moves in the third quarter of 1972 affected Brezhnev and Kosygin—the two cue takers in the core group most likely to be responsive to major developments in the international environment. These two leaders reacted quite differently. Suslov, in a position to shape a compromise, probably recognized the new fluidity among his colleagues and advanced a solution that received the support of Kosygin and all the others except Brezhnev. Brezhnev, then, appears to have acquiesced to the other oligarchs in the new decision to provide Egypt with the military support it needed for a war with Israel.

Conclusion

We address two puzzles. First, how do we account for the sudden shifts in the Soviet position on providing substantial military assistance to Egypt in 1972–73? The Soviet leadership in both word and deed had displayed great caution about giving Egypt offensive arms that would enable it to engage in another round of wars with Israel. Suddenly their position shifted. Why?

A second puzzle concerns Western scholarship on the Brezhnev era. It is widely acknowledged that some form of collective leadership characterized the top levels of policymaking during much of the Brezhnev period, particularly in the early years. Nevertheless, when it comes to actual analysis of the decision, the implications of collective leadership—that sharp disagreements between the leaders must have arisen on some major policy questions—seems to be ignored. Instead, in analysis of the Brezhnev period scholars follow one of several escape routes. For example, they assume policy is made by a single, unitary actor, so that disagreements among leaders need not be considered. Alternatively, policy is explained by reference to the position of only one individual, ignoring the fact that others must have been involved and may have advocated competing views. Still another practice has been to ignore the decision process entirely and suggest that Soviet behavior is a response to actions or policies directed toward the Soviet Union or a reaction to Soviet actions. We believe it is necessary to offer explanations consistent with our descriptions of the Soviet policy-making process.

Beyond proposing a means to cope with the puzzles we have noted, our purpose has been to advance a model of Soviet decision making that possesses several essential features. First, the model advances an explanation that merges variables from three levels of analysis—individual, group, and regime. They are not viewed as competing sources of explanation but as complimentary. Each one adds useful interpretive information that can be integrated with that from other levels of analysis.

Second, we have introduced a contingency approach to characterize the relationship among the variables. The relationships change under different circumstances. Thus, when regimes change, the decision rules for dealing with disagreements among Politburo members change. The decision trees presented as figures have served as one methodological vehicle for structuring contingencies.

Third, we have related decision structures and processes to outcomes or, more accurately, decisions. We have estimated positions for individual powerful leaders and have offered theory-based rules for suggesting which position will prevail and whether in its original or a modified form.

Finally, we have sought a general means of explanation rather than explanation constructed for one, and only one, occurrence in Soviet policy. We certainly have not tested the model here by interpreting a single case, but we hope to have demonstrated the plausibility of such a test. It has been our intention to fashion a model that employs variables for which we might reasonably acquire some empirical data—as we did in the Soviet decision to rearm Egypt—for a large class of actual decisions, thus facilitating investigation of the accuracy of the model systematically. A more refined version of the model, based on systematic research, should offer some short-term forecasting potential.

Perhaps a few words about data are important because we wish to keep the model in harmony with obtainable data. A recurrent criticism of decision-making approaches to the study of foreign policy has been the severe restrictions on acquiring meaningful data about the decision structures and processes of governments until years later when archives are opened, and even then the opportunities may seem limited. The alternative demonstrated in the data used for the Egyptian case has been to use public materials: content analysis of speeches and interviews to estimate attitudes and personal characteristics, biographical data to judge organizational experience, and thematic analysis of papers and journals associated with institutions to estimate organizational interests. We have also depended on expert judgments by academic and policy authorities. In using all these materials we have engaged in an inference process—often moving from the more general subject about which we had some indications to the particular decision in which we were interested. The possibility for error is obvious. But our process is reproducible. Others can follow our inference path, challenge it, and see how their alternative inferences change both a data point and—when plugged into the model—the expected outcome. It is a process for examining less accessible political leaders and governments that should gradually enhance our understanding as we compare the impact of competing, but explicit, inferences on the adequacy of our modeled explanations.

Notes

An earlier draft of this paper was presented at the annual convention of the International Studies Association, Washington, DC, 1987. The research reported in this paper was sponsored by the Mershon Center, Ohio State University. This paper—like the project from which it is drawn—represents a collaborative effort among the authors with Stewart preparing the materials on the regime, Margaret Hermann the materials on individual member choices, and Charles Hermann the materials on collective decision making. The authors would like to thank John Willieme for his help in collecting the case study materials and processing the attitude data reported in this paper.

1. We described these two variables in more detail in Stewart, Hermann, and Hermann 1986. for power to regime type. We are fully aware that our approach to regime does not address the question of how one regime changes into another. This issue is left to the further development of the model. However, we do believe that two of the most important variables whose interaction explains regime change are (1) perceived threats to such national values as system survival, or system stability and (2) perceived risks to organizational values. The sources of these risks may be inaction, proposed policy, or external events, to suggest only the most important. We are indebted to Randall Ripley for help in clarifying the concept of regime change.

2. Head (1982, 40) argues that the "presiding group" in Soviet foreign policy making throughout the period 1969–73 included "Brezhnev, Suslov and Ponomarev for the Party; Kosygin and Gromyko for the government; Andropov for the KGB; Grechko for the military." While Head's basic analysis is correct in terms of decision-making authority,

he ignores the important fact that prior to April 1973 Gromyko, Grechko, and Andropov were only candidate members of the Politburo. Hodnett (1981, 97) includes in the "inner core of leadership" in the 1970s, Kirilenko, Kosygin, Suslov, and Podgorny, in addition to Brezhnev. (See, also, Gelman 1984, 65–67 and Garthoff 1975, 29.)

3. We describe these four factors in more detail in Stewart, Hermann, and Hermann 1986.

4. We readily acknowledge that these data are fragile and that slippage may exist between public utterances and expressed positions in Politburo meetings. But they do provide one set of indicators that allow us to apply the model to an actual case and to check the fit of the model's expectations with what actually happened.

5. There is a growing literature in both political science and psychology examining how individuals process information. The references listed here provide useful summaries of this literature for both disciplines.

6. The accommodative and competitive interpersonal styles represent aggregates of individual traits. These styles and what comprises each of them are described in detail by M. Hermann (1980a, 1983). The data for interpersonal style also come from content analyses of the speeches and writings of these Politburo members.

7. We present the decision trees for the other regime types in Stewart, Hermann, and Hermann 1986.

8. Brezhnev's behavior here may reflect the traditional Soviet pattern of becoming silent in a period of policy reevaluation. Although Brezhnev and Kosygin reacted differently to Sadat's initiatives, this outcome is not inconsistent with what the model would predict. The model says the cue takers will show a change but does not yet indicate the direction of that change.

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Charles F. Hermann is Director and Philip D. Stewart and Margaret G. Hermann are Senior Faculty Associates, Mershon Center, Ohio State University, Columbus, OH 43210. They are also affiliated with the Department of Political Science, Ohio State University.

CRISIS STABILITY IN THE NUCLEAR AGE

ROBERT POWELL
Harvard University

In the logic of crisis stability, first-strike advantages may still be destabilizing, although even a successful first strike cannot protect a state from a terribly costly retaliatory second strike. If there is an advantage to striking first and if war seems sufficiently likely, launching a preemptive first strike may seem to be the least of evils. I reconsider the logic of crisis stability by studying games that are completely stable, although there are first-strike advantages. Four general conditions ensure stability. Identifying these conditions refines our understanding of crisis stability by also identifying the potential sources of instability, and this more refined understanding shows that the logic of crisis stability focuses attention too narrowly on first-strike advantages. Stability results from a more subtle interaction of several factors of which a first-strike advantage is only one.

Even if successful, a first strike cannot secure a state from a costly second strike. First-strike advantages are still destabilizing in the logic of crisis stability. If war seems almost inevitable and it is better to strike first than second, a state may find attacking in the hope of preempting an adversary's attack to be the best of a set of bad alternatives.

The logic of crisis stability was appreciated early in the development of nuclear deterrence theory. In *The Strategy of Conflict* (1960), Thomas Schelling examined one way that even modest first-strike advantages could be destabilizing. He studied a situation in which it is better to attack first than second if there were going to be a war. "But if the gains from even a successful surprise are less desired than no war at all, there is no 'fundamental' basis for an attack by either side. Nevertheless, it looks as though a modest temptation—too small by itself to motivate an attack—might become compounded through a process of interacting expectations, with additional motive for attack being produced by successive cycles of 'He thinks that we think that he thinks we think . . . he thinks we think he'll attack; so he thinks we shall; so he will; so we must' " (1960, 207). Schelling called the process through which successive expectational cycles might lead to an unlimited nuclear exchange "the reciprocal fear of surprise attack."

The logic of crisis stability has become a central tenet of deterrence theory and continues to provide the theoretical foundation for many policy analyses and for more general work. This logic is used, at least rhetorically, to evaluate arms control proposals and weapon systems. Proposals that limit or reduce counterforce capabilities are said to enhance crisis stability because they reduce first-strike advantages. Weapons like the U.S. MX and the Soviet SS-18 are criticized as being destabilizing because their counterforce capabilities contribute to the creation of first-strike advantages. And strategic defenses that are ineffective against a well-coordinated first-strike but are more effective against a ragged second-strike are destabilizing because they give an advantage to the side striking first (e.g., Drell, Farley, and Holloway 1984, 81). More generally, the logic of crisis stability is at the theoretical center of the growing literature on the significance of offense or defense dominance (e.g., Jervis 1978; Quester 1977; Sagan 1986; J. Snyder 1984; Van Evera 1984, 1985, 1987). The offense dominates the defense if it is better to strike first than second. The logic of crisis stability is therefore said to imply that an international system in which offense is dominant is less stable than one in which the defense is dominant.

I reconsider the logic of crisis stability. The analysis studies one example taken from a large set of games in which there are first-strike advantages. Because there is an advantage to striking first in these games, the logic of crisis stability would seem to imply that there will be some chance of escalating to a general nuclear exchange. Indeed, the larger the advantage, the greater the chance of an exchange. Yet, the games in this set are completely stable. The probability of a general nuclear exchange is zero even though there are first-strike advantages.

Four conditions ensure that the games in this set will be stable despite the existence of an advantage to striking first. First, a general nuclear attack may be the inadvertent consequence of escalation, but it cannot be purely accidental. Second, whenever a state can attack, it can also attempt to end the crisis by submitting to its adversary. If, moreover, its adversary has not already committed itself to launching a general nuclear attack, the crisis will end with the state's submission and there will not be a general nuclear exchange. Third, if there are two nuclear superpowers in the international system, it must be that neither state would launch a general nuclear attack unless it believes that the probability that a general nuclear war is inevitable is greater than 1/2. If there are M nuclear superpowers in the system, then it must be that no state will launch a general nuclear attack unless it believes that the probability that a general nuclear war is inevitable is greater than $(M - 1)/M$. Because the threshold needed to ensure stability rises as the number of nuclear superpowers in the system increases, a bipolar system may in this sense be said to be more stable than a multipolar system. The final condition needed for stability is that the states must fully appreciate their situation in that the first three conditions must be common knowledge.

Identifying the conditions needed to ensure stability refines our understanding of the problems of preemption and crisis stability. If a set of conditions ensures stability, then at least one of these conditions must be violated if there is to be any instability. Thus the general conditions that ensure stability are also the potential sources of instability. In this way, identifying the general conditions that ensure stability also serves to identify the potential sources of instability, and this refines our understanding by clarifying the relation among the multiple factors needed to ensure stability.

This more refined understanding, in turn, shows that the logic of crisis stability focuses attention too narrowly on the size of any first-strike advantages. The general conditions show that even within the context of rational actor game-theoretic models, instability results from a more subtle interaction of several factors of which a first-strike advantage is only one. By itself the existence of an advantage to striking first is not enough to create instability.

Stability with First-Strike Advantages

In his study of crisis stability in the nuclear age, Schelling (1960, 207–29) focused on a highly stylized description of this age that captured the essence of the problem of preemption. Because the description was highly stylized, it was analytically tractable. Because it captured the essence of the problem, its analysis held the promise of shedding light on the nature of crisis stability.

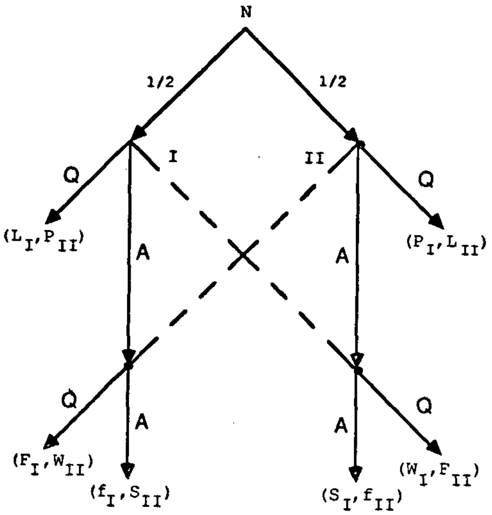
Three features characterize Schelling's stylization. First, there is "no fundamental basis for war" (Schelling 1960, 207); that is, a general nuclear war is so horrible that if a state had to choose between the certainty of war, even a war in which it were sure of having the first strike, or the certainty of peace, albeit a peace secured by submitting to its adversary, it would choose to avoid a general nuclear war. But second, if war is inevitable, it is better to strike first than second. There is a first-strike advantage. The first two characteristics of the stylization are, in sum, that a general nuclear war is so terrible that only being struck first is worse than striking first. The third characteristic is that both states fully appreciate their situation (Schelling 1960, 207–8).

I present a game-theoretic model of a situation that is consistent with this stylized description. The logic of crisis stability implies that because there are first-strike advantages in this situation, there should be some risk of preemption. But as will be shown, there is no risk. The game is completely stable although there is an advantage to striking first.

Before developing the model, however, it is important to recognize that Schelling and others have often cast the problem of crisis stability somewhat narrowly in terms of a general, all-out, or unlimited war (Schelling 1960, 1966 and Brams 1985; Brams and Kilgour 1987; Jervis 1978, 1979, 1984; O'Neill 1987; Powell 1987, 1988). But, of course, there are many forms of limited war, and there is no a priori reason to believe that factors that tend to enhance stability by making an unlimited war less likely also make limited war less likely. Indeed, the essence of the stability-instability paradox is precisely the proposition that as unlimited war becomes less likely, limited war becomes more likely (Jervis 1984; G. Snyder 1965). Stability must often be treated as a multidimensional notion. In keeping with the logic of crisis stability, however, stability will be interpreted narrowly as the probability of an unlimited war. (The implications of this narrow interpretation will be discussed more fully below.)

The game is illustrated in Figure 1. Play begins with Nature's *N*, making a random move: Nature takes the right branch with probability 1/2 and the left branch with probability 1/2. (Nature could just as well take the right branch with probability *p* and the left branch with probability 1 −

Figure 1. A Game of Preemption



$P>L>F>f>S>W$

p, and the following analysis and conclusions would be unaffected.) Beginning the game with a random move is a modeling technique that, as will be seen, formally creates the fundamental uncertainty that is the essence of the problem of preemption and of crisis stability.

If Nature plays left, state I is the first state in the game to move. I must decide between two options. It can quit and thereby end the game by playing Q. Ending the game at this point avoids an unlimited nuclear exchange, but it also brings I the loser's payoff L_I. II receives the payoff to prevailing of P_{II}. Or, I may decide to launch an unlimited nuclear attack, A. If I attacks, II must then choose between two options. Because I has already attacked, I is certain in the game to achieve a first strike. Nevertheless, II is not indifferent to its two options. Given that I has attacked, II can obtain a higher payoff by attacking rather than quitting. The underlying motivation here is that a prompt second strike, which may be thought of as II's playing A, given that I has attacked, is better than a slow second strike, which is represented by Q. Because II prefers A to Q, $S_{II} > W_{II}$ where S_{II} is the payoff to II of attacking immediately with its second strike and W_{II} is the payoff of not attacking and, in effect, waiting until later to launch a second strike. I, moreover, is not indffferent to II's decision. I prefers II to quit and thereby be able to launch only a less-effective second strike. That is, $F_I > f_I$.

If Nature plays to the right, II is the first state to move in the game and it must choose between Q and A. Now, however, when II is making its decision, I has not already committed itself to attacking. If II quits by playing Q, the game ends without there having been an unlimited nuclear exchange. II receives the loser's payoff of L_{II}, and I obtains the payoff to prevailing of P_I. If II attacks, it now achieves a first strike, and I must then decide what to do. Given that it has been attacked, I prefers attacking to quitting: $S_I > W_I$. Conversely, II would prefer that I did not attack: $F_{II} > f_{II}$.

To complete the specification of the relation among the payoffs, recall that in the stylization of the problem of preemption and crisis stability there is no fundamental basis for war. Although prevailing is better than losing, losing and thereby avoiding an unlimited nuclear exchange is better than the payoff to even a successful first strike (Schelling 1960, 208). Only being struck first is worse than striking first. In terms of payoffs, this means, $P > L > F$.[1] There are, however, first-strike advantages: $f > S$. Combining these gives the following relation among the payoffs: $P > L > F > f > S > W$.

The final step in describing the preemption game in Figure 1 is to discuss its information structure. Both of I's decision nodes are in the same information set. (This is illustrated in Figure 1 by connecting I's decision nodes with a dashed line.) Similarly, both of II's decision nodes are in the same information set. A player's information set represents its state of knowledge or uncertainty when it must make a given decision. More specifically, a player knows *whether* or not it is in a particular information set, but a player cannot determine *where* it is in a given information set. A player must therefore form beliefs about where it is in an information set, and these beliefs are in turn shaped by the other player's strategy. In the preemption game, I does not know if it is at the upper-left decision node, in which case it is the first to move in the game, or the lower-right node, in which case it is the second state to move. Similarly, II does not know if it is at the upper-right decision node, in which case it has the first move in the game, or the lower-left node, in which case it has the second move.

The states' uncertainty, as represented in the game by the states' information sets, formally creates the same dilemma as does the problem of preemption. Viewing the game from *I*'s perspective, suppose that when it had to choose between Q and *A*, *I* was certain that it was at the lower-right decision node. At this node, war is inevitable. *II* has already committed itself to attacking, and regardless of whether *I* chooses Q or *A*, *I* cannot avoid an unlimited nuclear exchange. In the stylization of the problem of preemption, a state will attack if it believes war is unavoidable. The same is true of the game in Figure 1: *I* attacks if it is certain that war is inevitable, that is, if it is certain that it is at the lower-right node, because $S_I > W_I$. Now suppose that when it had to choose between Q and *A*, *I* was certain that it was at its decision node at the upper left. Here, war is not inevitable. Indeed, *I* can avoid an unlimited exchange by quitting. In the stylization, the only fate worse than striking first is being struck first. So if a state is certain of being able to avoid an unlimited nuclear exchange by accepting the limited losses of submitting, a state will submit. The preemption game also captures this aspect of the stylization. If *I* is certain that it is at the upper-left node, then *I* will quit. Quitting brings L_I and the best that attacking can bring is F_I, where $L_I > F_I$.

The dilemma posed in the game, as in the logic of crisis stability, is that *I* is not sure that war is inevitable. *I* does not know whether it is at the upper-left node, in which case war is avoidable, or the node at the lower right, in which case war is inevitable and attacking is the best strategy. The more confident *I* is that war is inevitable, that is, the higher the probability *I* ascribes to being at the lower-right node, the higher the expected payoff to attacking and the lower the expected payoff to not attacking. At some threshold probability, *I* becomes sufficiently confident of the inevitability of war that the expected payoff to attacking is greater than the expected payoff to not attacking, and *I* attacks.

The essence of the logic of crisis stability is that there is some chance that this threshold will be crossed. The logic implies that in this situation there is some risk that one or both states will attack because it seems sufficiently likely that war is inevitable. But is this implication correct? More formally, is there any chance that in an equilibrium of the game in Figure 1 one of the states will attack? The answer to this question is *no*. With one further restriction on the states' payoffs, the probability that *I* or *II* will attack in equilibrium is zero. The situation is completely stable even though there is a first-strike advantage.

To show this it will be useful to transform the game tree of Figure 1 into a payoff matrix. Each state in the game tree has a single decision to make, whether to quit or attack. Moreover, this decision is made in ignorance of what the other state does. Accordingly, this situation can be represented by the 2 × 2 payoff matrix of the normal form game in Figure 2.

Figure 2. The Normal Form of the Preemption Game

		II	
		A	Q
I	A	$\frac{f_I+S_I}{2}$, $\frac{f_{II}+S_{II}}{2}$	$\frac{P_I+F_I}{2}$, $\frac{L_{II}+W_{II}}{2}$
	Q	$\frac{L_I+W_I}{2}$, $\frac{P_{II}+F_{II}}{2}$	$\frac{L_I+P_I}{2}$, $\frac{L_{II}+P_{II}}{2}$

$P>L>F>f>S>W$

The payoffs in the matrix in Figure 2 are derived from the tree in Figure 1. Suppose, for example, both states adopt the strategy Q. If Nature happens to play left, which it does with probability 1/2, then *I* moves first in the game. At this point *I*, according to its strategy, quits by playing Q. The game ends with payoffs (L_I, P_{II}). If Nature happens to play to the right, then *II* moves first. *II* then quits, and the game ends with payoffs (P_I, L_{II}). Thus the expected payoff to *I* if both states play Q is $(1/2)L_I + (1/2)P_I$. This is the payoff reported for *I* in the (Q,Q) cell of the payoff matrix in Figure 2. The other payoffs are calculated similarly.

In some circumstances, the probability of an attack is zero and the situation is completely stable despite the existence of first-strike advantages. To identify these circumstances, compare *I*'s strategy of attacking, which is represented by the top row of Figure 2, to *I*'s strategy of quitting, which is in the bottom row. Suppose that *I* were certain that *II* would play Q. Then *I*'s payoff for quitting is $(L_I + P_I)/2$ and for attacking is $(P_I + F_I)/2$. Because $L_I > F_I$, *I*'s payoff for quitting is greater than its payoff for attacking. If, therefore, *I* believes that *II* is playing Q, *I* would certainly not attack, for it is sure to do better by quitting. Now assume $L_I + W_I > f_I + S_I$. A justification for this assumption will be discussed below. At this point, simply note that if this assumption holds, then even if *I* believes that *II* is playing *A*, *I* prefers playing Q to playing *A*.

In sum, if $L_I + W_I > f_I + S_I$, *I* prefers Q to *A* regardless of what *II* does. For *I*, quitting strictly dominates attacking. Similarly, if $L_{II} + W_{II} > f_{II} + S_{II}$, *II* prefers Q to *A* regardless of what *I* does. *II* therefore never attacks. With $L_I + W_I > f_I + S_I$ and $L_{II} + W_{II} > f_{II} + S_{II}$, neither state attacks and there is no exchange in equilibrium. The situation is completely stable although there is an advantage to striking first.[2]

The crucial assumption that $L + W > f + S$ is a mathematical formalism but one that has two less formal, more intuitive, and more general interpretations. The first will be discussed here. The second will be addressed in the context of the discussion of why the logic of crisis stability may be misleading. To examine the first interpretation of $L + W > f + S$ it will be helpful to rewrite the expression as $(L - f)/(S - W) > 1$.

Both the numerator, $(L - f)$, and the denominator, $(S - W)$, of this latter expression have an intuitive interpretation. The numerator is the difference between the payoff for striking first and the payoff for quitting; that is, the numerator is the net cost of attacking and then suffering a devastating second strike when by quitting a state would have avoided an unlimited nuclear exchange. Similarly, the denominator is the net cost to a state of waiting to attack and thereby reducing the effectiveness of its second strike when by attacking immediately a state would have achieved a more effective second strike. The assumption $(L - f)/(S - W) > 1$ implies that the former is greater than the latter; that is, the net loss of striking first and thereby triggering a nuclear exchange that could have been avoided is greater than the net loss of launching a less effective rather than a more effective second strike.[3]

This game-theoretic model is clearly too simple to be accepted as model of a crisis. There are, for example, too few choices in the game and its information structure is much too simple. Yet the game satisfies the three stylized conditions that describe the problem of crisis stability and of preemption. First, there is an advantage to striking first. But second, because even a successful first strike does not guard a state from a second strike, only being struck first is worse than striking first. There is no fundamental basis for war. Finally, the states fully appreciate their situation. Thus the logic of crisis stability, if applied to this game, would seem to imply that because there are first-strike advantages, there will also be some risk of a state's becoming sufficiently certain of the inevitability of war to attack. Yet, as has been shown, no state ever attacks. Apparently, no state ever becomes sufficiently certain of the inevitability of war to justify attacking. The logic of crisis stability in this model is misleading. This raises two questions. First, why is the logic misleading? Second, how general is this result, that is, is there something special about this particular game that makes it stable despite the logic of crisis stability or are there more general conditions that ensure stability?

The Conventional Logic of Crisis Stability

The conventional logic of crisis stability —namely that first-strike advantages are destabilizing and that the greater the first-strike advantage, the less stable the crisis —underlies many analyses of both nuclear and nonnuclear crises. (This logic will be referred to as the "conventional logic of crisis stability" to distinguish it from the logic of crisis stability illustrated in the preemption game.) This logic is easily formalized and doing so will help to explain why it is misleading in the preemption game in Figure 1. If a state attacks preemptively its payoff is F. If a state does not attack, one of two outcomes is possible. If this state's adversary attacks, the state by not attacking condemns itself to suffering a first strike and retaliating with a second strike. Let S be the payoff to this outcome. If, however, neither the state nor its adversary attacks, then the states avoid a nuclear exchange. The payoff to this is C. In the stylization of the problem of preemption in the nuclear age, there is a first-strike advantage. So the payoff to striking first is greater than the payoff to striking second: $F > S$. But also in keeping with the stylization, even a successful first strike cannot protect a state from a second strike. Only being struck first is worse than striking first. In terms of payoffs, this means that $C > F$. Now let τ be the subjective probability that war is inevitable; that is, the state believes that the probability of its adversary's attacking is τ. Then the expected payoff of not preempting is $\tau S + (1 - \tau)C$.

Attacking preemptively is rational if its payoff is greater than the payoff to not preempting; that is, attacking is optimal if $F > \tau S + (1 - \tau)C$. Solving for τ gives $\tau > (C - F)/(C - S)$. Defining the threshold τ^* as $(C - F)/(C - S)$, attacking becomes the least of evils if a state believes that the probability that war is inevitable is greater than τ^*. That is, attacking is rational if $\tau > \tau^*$. Conversely, waiting and not preempting is optimal if a state's level of confidence is below this threshold.

The threshold at which attacking becomes rational is closely related to the notion of a state's critical risk (G. Snyder and Diesing 1977, 48),[4] which plays a crucial role in many models of crisis bargaining (Ellsberg 1959; Jervis 1972, 1978; G. Snyder 1971, 1972; G. Snyder and Diesing 1977). In critical risk models, which are often based on the game of chicken, a state prefers to submit if it is certain that its adversary will stand firm. Thus if the subjective probability that the adversary will attack is greater than the state's critical risk the state will submit. In models in which there is an advantage to striking first, however, a state prefers to attack if it is certain that its adversary will stand firm by attacking. Thus, if the subjective probability that war is unavoidable because the adversary will attack is greater than the threshold τ^*, the state will attack.

The essence of the conventional logic of crisis stability is captured in the relation between the threshold τ^* and the size of the advantage to striking first, $F - S$. To see this, rewrite the expression for τ^* to obtain $\tau^* = 1 - (F - S)/(C - S)$. Now note that as long as there is an advantage to striking first, that is, as long as $F - S > 0$, the threshold τ^* is less than one. Thus it is always possible that the probability that measures the strength of a state's belief that war is unavoidable—τ—can exceed this threshold and thereby make attacking rational. As long as there is an advantage to striking first, there is some probability that makes attacking the least of evils. Moreover, the higher the payoff to striking first or the lower the payoff to being struck first, the greater the first-strike advantage and the lower the threshold (that is $\partial\pi^*/\partial F < 0$ and $\partial\varrho^*/\partial S < 0$). And the lower the threshold, presumably, the easier it is to exceed and, consequently, the less stable the crisis. Finally, suppose that as in the strictest interpretation of the doctrine of mutually assured destruction (MAD), there is no advantage to striking first; that is, $F - S = 0$. Then the value of the threshold τ^* is one and can never be crossed. Crises in this strictest interpretation of the doctrine of MAD are very stable.

The formalization of the conventional logic of crisis stability reveals a peculiar aspect of this logic, and this is the source of the conventional logic's misleading conclusions. First-strike advantages mean that τ^* is less than one, and this creates instabilities by making it possible that τ may be greater than τ^*. But note that the conventional analysis treats τ and τ^* quite differently. τ^* is a function of the parameters C, F, and S, which describe the structure of the crisis. The threshold τ^* is endogenously determined by the structure of the crisis. But τ is not an explicit function of any parameter that describes the crisis. τ is formally exogenous.

Treating the strength of a state's belief that war is inevitable as formally exogenous is problematic, for this treatment is at odds with the notion of a crisis as a bargaining process. Many studies of crises see them as bargaining processes (e.g., Brams 1985; Brams and Kilgour 1987; Ellsberg 1959; Huth and Russett 1984; Jervis 1972, 1978; Lebow 1981; Powell 1987, 1988; Schelling 1960, 1966; G. Snyder 1971, 1972; G. Snyder and Diesing 1977; Wagner 1982). But the essence of bargaining is attempting to shape an adversary's beliefs. A state tries to alter its adversary's assessment about the relative costs and benefits of continuing the confrontation in the hope that the adversary will eventually accept an agreement more favorable to the state. In a crisis in the nuclear age, for example, each superpower knows the other is capable of destroying it. A crisis is less about what one can do than about what one is willing to do. Each state attempts to further its ends by demonstrating its resolve, that is, by influencing its adversary's beliefs about what it is willing to do. Inherent in the notion of a crisis as a bargaining process is the presumption that a state's beliefs are affected by its adversary's actions; that is, beliefs are endogenous. And, indeed, beliefs are often discussed as if they were endogenous (e.g., Ellsberg 1959; Huth and Russett 1984; Jervis 1972, 1978; Lebow 1981; G. Snyder 1971, 1972; G. Snyder and Diesing 1977). Yet in the conventional logic of crisis stability beliefs are not related to the crisis formally.[5]

The formally exogenous treatment of beliefs raises questions that reflect on the problem of stability. Could structural features of a crisis constrain a state's belief that war was unavoidable? Could such constraints keep this belief from becoming strong enough to justify attacking? Could, for example, the fact that even a successful first strike cannot protect a state from a terribly costly second strike keep τ below τ^*? If so, the crisis would be stable despite the existence of first-strike advantages.

As shown above, the preemption game in Figure 1 is completely stable. The reasons for this are twofold. First, the model allows the states' beliefs to be treated endogenously. Second, when these beliefs are so treated, neither state becomes sufficiently confident that war is inevitable to justify its attacking.

Showing that no state becomes sufficiently confident to attack takes two steps. The first is to find the maximum value that the probability that war is inevitable can attain in the model. The second step is then to establish that the threshold at which attacking becomes rational is still higher than this maximum. Because the probability that war is inevitable is always less than this maximum and this maximum is in turn below the threshold, the probability that war is unavoidable never crosses the threshold. In effect, τ is always less than τ^*. The crisis is stable even though there is an advantage to striking first.

To find the maximum value that the probability that war is inevitable can attain, consider *I*'s information set in ability is equal to the probability of Nature's playing left—1/2—times the probability of being at the lower-right decision node. The maximum value the probability that war is unavoidable can attain is then the highest probability that *I* can ascribe to being at this node. This probability is equal to the probability of Nature's playing left—1/2—times the probability of *II*'s attacking. This latter probability must be less than or equal to one. Thus the maximum probability that *I* is at the lower-right node and therefore that war is inevitable is 1/2. A similar argument shows that from *II*'s perspective the maximum probability that war is inevitable is also 1/2. In terms of the conventional logic, $\tau \leq 1/2$.

Because neither *I* nor *II* believes that the probability that war is inevitable is ever greater than 1/2, neither will attack and the crisis will be completely stable if the threshold probability at which attacking becomes rational is greater than 1/2. In terms of the conventional logic, the crisis will be completely stable if $\tau^* > 1/2$. To demonstrate that $\tau^* > 1/2$, it will suffice to show that a state would not attack if the probability that war is inevitable is equal to 1/2. If a player would not attack when this probability is 1/2, the threshold at which attacking becomes the least of evils must be still higher at some level above 1/2.

It is not difficult to see that *I* would not attack if the probability that war is unavoidable is equal to 1/2. If the probability of reaching *I*'s lower-right decision node is to be 1/2, then *II*'s strategy must be to play *A*. Given *II*'s strategy, *I*'s expected payoffs to playing *Q* or *A* may be calculated. The payoff for *Q* is $(L_I + W_I)/2$, and the payoff for *A* is $(f_I + S_I)/2$. With these payoffs, *I* will attack if $f_I + S_I > L_I + W_I$ and will quit if $f_I + S_I < L_I + W_I$.

Recall that it was assumed that $f_I + S_I < L_I + W_I$. Thus when *I* believes that the probability that war is inevitable is equal to 1/2, the assumption $f_I + S_I < L_I + W_I$ is precisely what is required to ensure that *I* will not attack. Similar reasoning also shows that if $f_{II} + S_{II} < L_{II} + W_{II}$, *II*'s threshold is also greater than 1/2.

Again the assumption that $f + S < L + W$ is crucial. Previously, this assumption was interpreted in terms of states' payoffs: the net loss due to suffering a retaliatory second strike in a general nuclear exchange that could have been avoided is larger than the net loss of launching a less-effective second strike. This assumption also has a second interpretation in terms of beliefs and thresholds, namely, that the payoffs in the model and, by analogy, in the crisis are such that no state would launch a general nuclear attack unless it believed that the probability that war was inevitable was greater than 1/2. (The fact that the threshold is 1/2 is not the result of Nature's playing the right branch at the beginning of the game with probability 1/2. If, for example, Nature played to the right with an arbitrary probability p and to the left with probability $1 - p$, the threshold needed to ensure stability would remain 1/2.)

This result contrasts sharply with the flavor of the conventional logic. In that logic if the first-strike advantage is small, that is, if $F - S$ is small, the threshold τ^* is close to one and, presumably, difficult to cross. The crisis even in the conventional logic is relatively stable. The preemption game in Figure 1, however, suggests that the threshold needed to ensure stability may be much lower than one and much lower than that suggested by the conventional logic. Indeed, a first-strike advantage that lowers the threshold to what by the conventional logic would seem to be a quite unstable level—say $\tau^* = .6$—may still be greater than 1/2 and thus still high enough to ensure stability in the model.

In sum, the conventional logic of crisis stability is misleading if applied to the preemption game in Figure 1 because that logic ignores the structural features of a crisis that may constrain the states' beliefs that war is inevitable. When these features are taken into account, at least in the context of the specific model examined above, these beliefs are constrained to be below the threshold at which attacking becomes rational. Despite the implications of the conventional logic, there is no risk of crossing this threshold. Although there are first-strike advantages, the situation is completely stable.

The General Conditions of Stability and Potential Sources of Instability

The stability of the preemption game in Figure 1 raised two questions. The first was why the conventional logic was misleading in this example, and this was attributed to its failure to treat beliefs as formally endogenous. The second question asks whether there is something very particular about the preemption game that ensures stability or whether some more general conditions are responsible for stability. Powell n.d. formally identifies a set of conditions that do ensure stability. The probability of an unlimited nuclear attack in equilibrium is zero in any game satisfying these conditions. I will here analyze these conditions less formally.

Identifying the conditions needed to ensure stability refines our understanding of the problem of preemption and of crisis stability. If a set of conditions ensures stability, at least one of these conditions must be violated if there is to be any instability; that is, the general conditions that ensure stability are also the potential sources of instability. In this way, then, identifying the general conditions that ensure stability also serves to identify the potential sources of instability, and this refines our understanding by clarifying the relation among the factors needed to ensure stability. This more refined understanding indicates that the conventional logic focuses attention too narrowly on the size of any first-strike advantages. The general conditions show that even within the context of rational actor game-theoretic models, instability results from a more subtle interaction of several factors of which a first-strike advantage is only one. By itself, the existence of an advantage to striking first is not enough to create instability.

Four conditions assure stability;[6] that is, as long as any game-theoretic model satisfies four general conditions, *even if there is an advantage to striking first*, the probability of there being an exchange in equilibrium is zero. The first condition is that there be no risk of a purely accidental general nuclear attack; that is, this attack cannot happen unless a state orders it. In the preemption game, for example, Nature cannot launch an attack directly (which would be at least one way to model a purely accidental attack).[7] Only the states can attack.

The preclusion of a purely accidental general nuclear attack, however, does not preclude the possibility of other types of accident. Rather, only one very narrow type of accident has been excluded. A general nuclear attack cannot result directly from an accident. This attack cannot be triggered automatically by a false alarm caused by, for example, a rising moon, a flock of geese, the failure of a computer chip (Bracken 1983, 48–49), or the loading of a practice tape simulating a Soviet attack onto the on-line system. Even if a purely accidental general nuclear attack is precluded, accidents and false alarms may certainly still occur. But they can only lead to this attack indirectly by creating a situation in which the National Command Authority accepts the risk that it may be acting on the basis of a false alarm and decides to attack deliberately.

It is important to emphasize that excluding the possibility of a purely accidental general nuclear exchange does not eliminate the possibility of an inadvertent general nuclear exchange. It is, moreover, the risk of an inadvertent exchange and not of a purely accidental exchange which is the essence of the problems of preemption and of the conventional logic of crisis stability. A general nuclear attack is purely accidental if it happens even though the National Command Authority in the attacking state did not order the attack. In an inadvertent attack, however, the command authority does order the attack. Escalation has put it in a situation in which attacking has become the least of evils. The attack is inadvertent because the command authority would have pursued a different course earlier in the crisis had it known that pursuing the original course would lead to a situation in which it deliberately attacked. An inadvertent general nuclear attack is deliberate, intentional, and therefore not purely accidental. But finding oneself in a position in which one intentionally attacks is an unintended consequence of escalation. The essence of the conventional logic of crisis stability is the risk of an inadvertent nuclear exchange. This is clear from the conventional logic's emphasis on the threshold at which attacking becomes rational and on the risk of crossing this threshold. The problem in the conventional logic is one of an inadvertent—not a purely accidental—exchange. Excluding the possibility of a purely accidental nuclear attack does not affect the essence of the problem.

The second assumption needed to ensure stability is that whenever a state has the option of attacking, it also has the option of trying to submit to its adversary. If, moreover, a state tries to submit before its adversary has committed itself to attacking, the crisis will end with the state's submission and without an unlimited nuclear exchange.

The preemption game in Figure 1 satisfies this condition. Whenever a state has the option of attacking, it also has the option of trying to submit by playing *Q*. Now suppose that *I* is actually at its upper-left decision node. (Of course, *I* is unsure if it is at this node or the one at the lower right.) At this node, *II* has not yet committtted itself to attacking. In keeping with the second condition needed for stability, if *I* attempts to submit here, the crisis will end without an exchange.

The option to attempt to submit is crucial to stability. But this option is a theoretical construct in an abstract game. It is an assumption about a model and may or may not be a sensible assumption to make about an actual crisis. Indeed, the stabilizing effects of such an option can be appreciated without asserting that such an option exists in any particular crisis. As noted above, the most important function of identifying the conditions that are sufficient to ensure stability may be that doing so also serves to identify potential sources of instability, and, especially, those that have not been identified previously.

Nevertheless, it is interesting to ask if this theoretical construct has any empirical referents in actual crises. For what, in other words, is this construct an apt analogy? The essence of having this option is not, as the preemption game in Figure 1 may have suggested, that the game must end immediately if a state tries to submit and its adversary has not already attacked. As the discussion of the dampening effects of escalation suggests, the essence of being able to submit if one's adversary has not already attacked is rather that each state has an option with two properties. First, the exercise of this option must be *evident* to one's adversary. Second, if an adversary observes that this option has been exercised, attacking is not in this adversary's best interest. If an actual option is to be an empirical referent corresponding to the theoretical construct, it should satisfy these two criteria.

Whether any real options do meet these criteria depends very much on the actual capabilities of the superpowers' strategic arsenals and their command systems. But suppose, for example, that to launch an effective first strike, a state's strategic forces must be brought to a higher state of readiness than is normally maintained. Moreover, each state is able to monitor the other's alert status and will know if its adversary is generating its strategic forces. Now suppose that in the midst of a crisis, an adversary either does not increase the readiness of its strategic arsenal or reduces it, and a state observes its adversary's failure to generate its forces. The state then reasons that such a step would make no sense if its adversary were planning to attack. But the only justification for the former to attack would be that it expected its adversary to attack. Accordingly, this state will not attack, although it may maintain the pressure on its adversary by not reducing its alert status or through other means. Given the suppositions that the alert status of the states can be monitored and that an effective first strike requires that forces be brought to a high state of readiness, an empirical referent corresponding to the theoretical construct of being able to attempt to resign to avoid a nuclear exchange if one's adversary has not already attacked would seem to be the *ability to maintain an alert status that* ensures that a devastating second strike can still be launched but that an effective first strike cannot be.

In this light it is interesting to note that during the Cuban missile crisis the Soviet Union apparently did not put its strategic forces on alert.[8] As air force general David Burchinal described Khrushchev's position in the crisis, "We put a gun to his head and he didn't move a muscle" (Trachtenberg 1985, 161). The Soviets, it would seem, not only possessed but exercised an actual option corresponding to the theoretical construct. Indeed, in many crises in the nuclear era, including the most severe crises over Berlin and, later, over missiles in Cuba, U.S. leaders, at least, were not significantly concerned by the prospect of the Soviets attempting to launch a first strike, and there was little reciprocal fear (Betts 1987, 164).

Finally, it should be emphasized that although escalation may not amplify the reciprocal fear of surprise attack, it may send other signals. These may include a willingness to inflict and endure severe but limited punishment in order to have one's way. Escalation may also enhance one's military position and thereby enhance the prospects of prevailing in any limited contest of military strength. These effects—not the fear of a general nuclear exchange—may convince an adversary to come to terms. Escalation remains an important means of exerting coercive pressure, although perhaps not for the reasons commonly assumed.

The third condition needed to ensure stability is that no state will attack unless it believes that the probability that war is inevitable is greater than 1/2. This condition in turn implicitly imposes a restriction on the underlying payoffs in the game and especially on the relative size of the first-strike advantage. Indeed, as was shown above for the preemption game in Figure 1, the restriction on the payoffs needed to ensure stability, that is, $L - S > f - W$, could be interpreted either explicitly as a direct restriction on the payoffs or as an implicit restriction on the payoffs requiring that no state would attack unless it believed that the probability that war was inevitable was greater than 1/2.

Two points should be underscored. First, the size of any first-strike advantage is not irrelevant to stability. If the advantage to striking first is so large that a state would attack even if it believed that the probability that war was inevitable is less than 1/2, there may be an unlimited exchange even if all of the other conditions needed to ensure stability are satisfied. It is, however, surprising that the threshold needed to ensure stability is so low. In the model in Figure 1, for example, a crisis will be completely stable if the threshold of inevitability at which it becomes rational to attack is, say, 60%. But viewed from the perspective of the conventional logic, a crisis in which the threshold was this low would most likely be described as highly unstable.

The second point to be emphasized is that the threshold value of 1/2 is not related to the fact that in the preemption game in Figure 1 Nature plays a particular branch at the beginning of the game with probability 1/2. Rather, the threshold is related to the number of nuclear superpowers. If there are two superpowers, the threshold is 1/2. But if there are M nuclear superpowers, the threshold of inevitability is $(M - 1)/M$; that is, to ensure stability the payoffs and first-strike advantages must be such that no state would attack unless the probability that war was unavoidable was greater than $(M - 1)/M$.[9] Note that as the number of nuclear superpowers increases, the threshold needed to ensure stability rises. In this sense, then, stability decreases as the number of nuclear superpowers increase, and this of course is suggestive of Waltz's (1979) argument that a bipolar system is more stable and less prone to war than is a multipolar system.

The fourth condition needed to ensure stability is the formalization of the third feature that characterized Schelling's stylization of the problem of preemption and crisis stability in the nuclear age. He assumed that the states fully appreciated their situation. More formally, the fourth condition is that the first three conditions are common knowledge; that is, every player knows that the first three conditions are met, that the other players know this, that all the players know that all the players know this, and so on. The first three conditions describe an objective situation. But as has been pointed out many times (e.g., Jervis 1982), deterrence depends on beliefs about a situation rather than on the actual situation. The same is true in game-theoretic analyses of deterrence. For example, if in the preemption game the actual payoffs satisfied $L - S > f - W$ but the states believed that this condition was not met, the game used to model this situation might not satisfy the first three conditions and there might be some chance of an exchange. The common knowledge assumption is crucial because it links the state's beliefs about the situation to the situation.[10] The states are assumed to understand fully the objective situation described by the first three conditions.

In sum, four general conditions ensure that the game and, by analogy, a crisis are stable despite the existence of an advantage to striking first. But because there are first-strike advantages in this game, the conventional logic of crisis stability would imply that there is some chance of an unlimited exchange and that the larger the advantage to striking first, the greater the chance of an exchange. Yet, the game and, by analogy, the crisis are completely stable despite these first-strike advantages. The conventional logic of crisis stability focuses too narrowly on first-strike advantages. Stability is the result of a more subtle interaction of several factors of which the size of any first-strike advantage is only one.

A final qualification about limited options is in order. If any game satisfies the first, second, and fourth assumptions, any option that satisfies the third assumption will not be played in equilibrium. Of course in games that are more complicated than the preemption game in Figure 1 there may be many more options than *A*, and these other alternatives need not satisfy the third assumption. In addition to launching an unlimited attack by playing *A*, there may, for example, also be an option *A*′, which corresponds to launching a limited attack. Depending on the underlying payoffs, it may very well be that unlike *A*, a state would launch *A*′ even if it were still quite confident that general war was not inevitable. If so, *A*′ would no longer satisfy the third assumption, and a state may play *A*′ in equilibrium (although it remains the case that *A* will not be played). The class of games defined by the four stabilizing conditions includes games in which there are limited options. But unfortunately, the preceding analysis is silent on the question of the effects of first-strike advantages on the likelihood of limited attacks. Whether or not these effects can be analyzed only in the context of specific games or whether there are any more general results remains an open question. Accordingly, it is important to remember that crisis stability is interpreted narrowly here, as elsewhere (e.g., Brams 1985; Brams and Kilgour 1987; Jervis 1978, 1979, 1984; O'Neill 1987; Powell 1987, 1988; Schelling 1960, 1966), as relating to the probability of a general or unlimited exchange.

Conclusion

In the logic of crisis stability, first-strike advantages may still be destabilizing even though a successful first strike cannot protect a state from a terribly costly retaliatory second strike. If war seems almost inevitable and if it is better to strike first rather than second, then attacking in the hope of preempting an adversary's attack may begin to appear to be the least of evils. When there are first-strike advantages and war seems very likely, a state may find launching a preemptive first strike to be in its best interest.

This essay has reconsidered this logic by studying a game in which there is no chance of a general exchange. The game is completely stable despite the existence of first-strike advantages. Four conditions ensure stability. First, a general nuclear attack may be the inadvertent consequence of escalation, but it cannot be purely accidental. Second, whenever a state can attack it can also attempt to end the crisis by submitting to its adversary. If, moreover, its adversary has not already committee itself to launching a general nuclear attack, the crisis will end with the state's submission and there will not be a general nuclear exchange. Third, if there are two nuclear superpowers in the international system, then neither state would launch a general nuclear attack unless it believes that the probability that a general nuclear war is inevitable is greater than 1/2. If there are *M* nuclear superpowers in the system, then no state will launch a general nuclear attack unless it believes that the probability that a general nuclear war is unavoidable is greater than $(M - 1)/M$. Because the threshold needed to ensure stability rises as the number of nuclear superpowers in the system increases, a bipolar system may in this sense be said to be more stable than a multipolar system. The final condition needed for stability is that the states fully appreciate their situation in that the first three conditions are common knowledge.

This analysis refines our understanding of the problem of preemption and of crisis stability. Identifying the general conditions needed to ensure stability also identifies the potential sources of instability. If there is to be any instability, at least one of the four conditions needed to ensure stability must be violated. Even within the context of rational-actor game theoretic models, instability results from a more subtle interaction of several factors of which the existence of a first-strike advantage is only one. The conventional logic's focus on first-strike advantages is too narrow.

Notes

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1. Throughout, the subscripts will be dropped when the relation applies to both *I* and *II*.

2. In this game where Nature plays the right branch with probability 1/2, stability would be assured if $L + W > f + S$ were assumed for only one state. But in a more general game in which Nature could play the right branch with probability p, $L + W > f + S$ would have to be assumed for both states in order to ensure stability.

3. This would not seem to be a very demanding assumption in the nuclear era. The superpowers' large and relatively survivable strategic arsenals would seem to ensure that even a successful first strike could not deny an adversary the ability to retaliate with a devastating second strike. The net cost of achieving a first strike in an otherwise avoidable exchange is large compared to the cost of a less-effective rather than more-effective second strike.

4. The precise formula for calculating a state's critical risk varies. For a discussion of this see Wagner 1982 and Powell 1987 (p. 722).

5. In treating beliefs exogenously what has become the conventional logic of crisis stability—although perhaps simpler—fails to capture much of the subtlety and insight of Schelling's original analysis of the problem (1960, 207-29), of which an essential element was treating beliefs endogenously.

6. A few additional technical assumptions are also needed. See Powell n.d. for a discussion of these as well as of some of the consequences of relaxing the major conditions.

7. Allowing for a purely accidental nuclear attack is the essence of Schelling's (1960, 1966) threats-that-leave-something-to-chance, and modeling this by letting Nature attack has provided the foundation of recent formal models of brinkmanship (Nalebuff 1986; Powell 1987, 1988).

8. Trachtenberg (1985, 156-61). See, however, the discussion of Soviet submarines in the Caribbean during the crisis in Sagan 1985 (pp. 112-18).

9. See Powell n.d. for the derivation of this result.

10. More precisely, the common knowledge assumption ensures that the extensive game representing the crisis—especially the states' uncertainty—satisfies the first three assumptions.

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Robert Powell is Assistant Professor of Government, Harvard University, Cambridge, MA 02138.

THE ABUSE OF PROBABILITY IN POLITICAL ANALYSIS: THE ROBINSON CRUSOE FALLACY

GEORGE TSEBELIS
University of California
Los Angeles

The decision to stay at home when you have no umbrella and rain is probable is an appropriate problem for decision theory. The decision to speed when you are in a hurry and the police might be patrolling is a game against a rational opponent. Treating the latter like a problem for decision theory is what I call the Robinson Crusoe fallacy. It is quite common and leads to incorrect conclusions. If the game has no pure strategy equilibrium, changes in the payoffs to a player affect not that player's strategy but the strategy of the opponent in equilibrium. For example, modifying the size of the penalty does not affect the frequency of crime commitment at equilibrium, but rather the frequency of law enforcement. I provide examples of this fallacy in regulation, international economic sanctions, and organization theory and argue that it stems from inappropriate use of the concept of probability.

> War is not an exercise of the will directed at inanimate matter. . . . In war, the will is directed at an animate object that reacts. It must be obvious that the intellectual codification used in the arts and sciences is inappropriate to such an activity.
>
> Clausewitz, *On War*

Robinson Crusoe was stranded on a desert island. He behaved as if he were the only person on the island until one day he discovered a footprint on the sand. From that moment he realized that he was not playing against nature any more. He was facing rational opponents, so he started fortifying his house to resist attacks. Clausewitz' statement indicates that mistaking a rational opponent for nature is an "inappropriate activity." I will call it a fallacy—the Robinson Crusoe fallacy.

My purpose is to generalize Clausewitz' statement for games that are not zero sum and to show that the Robinson Crusoe fallacy can lead to important mistakes. I will show that the expected utility calculations typically used in decision theory are inappropriate when probabilities are not exogenous but are part of the (equilibrium) strategy of a rational opponent. Mistaking the equilibrium mixed strategy of the opponent for a probability distribution is not an innocuous simplification. It leads to such important mistakes as the belief that modification of the payoffs of one player will lead to modification of that player's behavior. In reality, it leads to the modification of the (equilibrium) strategy of the opponent. I will provide examples of the Robinson Crusoe fallacy from the literature concerning crime, regulation, executive-legislative relations, international economic sanctions, and organization theory.

I first present two problems to which decision theory is traditionally applied: the decision to stay home or go out when rain is probable and the decision to violate or not violate traffic laws in the probable presence of the police. The former scenario correctly belongs to the domain of decision theory, while the latter is an example of the Robinson Crusoe fallacy and should be studied in a game-theoretic framework. Then I analyze the traffic violation example as a game-theoretic problem, demonstrating that the conclusions of decision theory are inappropriate, and those of game theory are counterintuitive. Finally I present examples of the Robinson Crusoe fallacy and put forward the argument that the concept of probability very often represents mixed strategies, with the result that the use of decision theory is inappropriate and may lead to wrong conclusions.

Rain and Crime: Decision Theory and the Robinson Crusoe Fallacy

Consider the following problem: rain is probable and you have no umbrella. How will you proceed in making the decision to stay at home or not?

According to Savage's (1954) terminology, there are two possible states of the world: rain and no rain. There are also two possible acts: to stay at home or to go out. Therefore, there are four possible outcomes: (a) you go out and get wet, (b) you go out and stay dry, (c) you stay home while it rains, and (d) you stay home while it does not rain.

Table 1 represents the utilities of these four possible outcomes. It is reasonable to assume that you prefer to go out if it does not rain. It is also reasonable to assume that you probably prefer to stay in if it rains. Formally, these assumptions can be stated, $b > d$ (A1) and $c > a$ (A2). In addition, it is very likely that there is a preference for rain if you stay in, justifying your choice ($c > d$). This assumption, however, is not necessary for what follows.

Table 1. Payoffs in the Rain Decision Problem

Decision	Rain	No rain
Go out	a	b
Stay in	c	d

Note: There are two states of the world (rain, not rain) and two acts (go out, stay in). Payoffs: $c > a$ and $b > d$.

In addition to assumptions A1 and A2, you need the probability of rain in order to make up your mind. Suppose that the weather report this morning gave you a reliable estimate p of this probability. Elementary decision theory dictates how to make your choice: you compare the expected utilities (EU) from each act and choose the act with the higher expected utility. In formal terms, you should go out if

$$\text{EUout} - \text{EUin} = [a \times p + b \times (1-p)] - [c \times p + d \times (1-p)] > 0.$$

In the opposite case, you should stay in.

In a series of similar situations, the probability that you will go out increases when the probability of rain decreases. Moreover, the probability that you will go out varies with your utilities: it increases with a and b, and decreases with c and d. Therefore, if you do not mind getting wet (a is high, since a is negative), the probability that you will go out increases. Similarly, if you intensely dislike staying in while it does not rain (low d, since d is negative), the probability that you will go out increases.

Consider another similar situation: you are driving your car and you are in a hurry. However, you do not know whether you should take the risk and exceed the speed limit. There are two states of the world: either the police are nearby or they are not. There are two acts to choose from: either to violate the speed limit or to abide by the law. Again, there are four possible outcomes: (a) you can get a ticket for speeding, (b) you can get to work on time without any incident, (c) you can arrive late and avoid a ticket, and finally (d) you can arrive late through there were no policemen on the streets.

Table 2. Payoffs in the Crime Decision Problem

Decision	Police	No police
Speed	a_1	b_1
Not speed	c_1	d_1

Note: There are two states of the world (police, no police) and two acts (speed, not speed). Payoffs: $c_1 > a_1$ and $b_1 > d_1$.

Table 2 represents the utilities of these four possible outcomes. It is reasonable to assume that you prefer to speed if there are no police around. It is also reasonable to assume that you prefer not to speed if the police are out in force. Formally, these assumptions can be stated, $b_1 > d_1$ (**A1′**) and $c_1 > a_1$ (**A2′**). Again, even though it is very likely that there is a preference regarding the state of the world when speeding or observing the law, such assumptions are not necessary.

Aside from assumptions A1′ and A2′, the probability that the police are lurking nearby is necessary to know in order to arrive at a decision. Suppose an estimate p of this probability is obtained from experience. Elementary decision theory dictates how the choice is made. The expected utilities from each act are compared and the act with the higher expected utility is chosen. In formal terms, you should speed if

$$\mathrm{EU}_{\mathrm{speed}} - \mathrm{EU}_{\mathrm{not}} = [a_1 \times p + b_1 \times (1-p)] - [c_1 \times p + d_1 \times (1-p)] > 0.$$

In the opposite case, you should observe the speed limit.

In a series of similar situations, the probability of violating the law increases when the probability of lurking policemen decreases. Moreover, the probability of speeding varies with your utilities: it increases with a_1 and d_1 and decreases with b_1 and c_1. In particular, the size of the penalty for speeding will influence the decision to speed. A higher fine for speeding will decrease the frequency of violating the law.

The use of decision theory in describing a problem that "obviously" belongs to the domain of game theory, since nature and the police (a rational agent) are quite different in their behavior, is inappropriate. In other words, I have committed the Robinson Crusoe fallacy. However, this is a crude and oversimplified—yet faithful—account of the logic of what is known in the literature as the economic approach to crime, or deterrence theory. For authoritative accounts of these theories, refer to Becker 1968, Stigler 1970, Ehrlich 1973, and Ehrlich and Brower 1987, in which the conclusions of my simplified example—in particular the deterrent effect of the size of the penalty on crime—are stated and proven.

Why is a decision-theoretic approach appropriate for the problem of rain and inappropriate for the problem of crime? The obvious reason is that nature can reasonably be approximated by a probability distribution, while a rational player cannot. This answer pushes the problem a bit further: can we approximate a rational player's expected actions by a probability distribution?

Axiom P2 of Savage's account of expected utility theory provides the answer. This axiom expresses formally "the sure thing principle,"[1] and assumes that states of the world are independent of the acts chosen by the decision maker.

Take Newcomb's paradox (Eells 1982) as an example to demonstrate the meaning of independence between acts and states of the world. Suppose there are two boxes, one open and one closed. There is a thousand dollars in the open box, while the closed box contains either a million dollars or nothing. You are presented with the option of either taking the contents of both boxes, or only taking the contents of the closed box. There are two states of the world (the closed box has either a million dollars or is empty) and two acts from which to choose (either take both boxes or only the closed one).

There is a dominant strategy in this problem: take both boxes, because in every possible situation this choice offers a thousand dollars more. Formally, if we assign probability p to the situation where the closed box contains a million dollars, for any value of p in the [0,1] interval

$$EU_{both} - EU_{closed} = p \times 1000 + (1 - p) \times 1000 = 1000.$$

Now consider Newcomb's complication: assume you are informed that a demon decides the contents of the closed box; if it expects you to pick up only the closed box it puts a million dollars inside. If it expects you to be greedy and pick up both boxes, it places nothing in the box. Informed that the experiment has been performed a million times and that people *always* left either with a million dollars or with a thousand dollars, what will you do?

One possible line of reasoning is that whatever the contents of the closed box, those contents already have been decided by the demon and therefore your decision will have no effect. Consequently, you are better off if you choose both boxes. This reasoning applies the "sure thing principle" (Savage's axiom P2), a principle that chooses to ignore information that may be relevant. The second possible line of reasoning is that it is almost certain (because of the regularity of the results of past experiments) that the demon is always right in its anticipation: you will finish with a thousand dollars if you choose the dominant strategy and a million dollars if the dominated strategy is chosen. This reasoning ignores the "sure thing principle" because it assumes that states of the world are dependent on the acts chosen.

Put the insights generated by this (unrealistic) thought experiment in more plausible settings and consider the following questions:

1. Does the probability of rain change if people go out or stay in?
2. Does the probability of police enforcement change if people abide by the law or violate it?

The answer to the first question is *no*, while the second is *yes*. Indeed, the police will enforce the law if it is violated, preferring to relax if there is no reason to be present.[2] In other words, the conditional probability that the police will enforce the law—given that it is violated—is different from the unconditional probability of law enforcement. Thus, we cannot replace the action of the police by a probability distribution *independent* of the acts chosen by the people. Robinson Crusoe is not alone, doing the best he can against nature. We have to introduce Friday into the picture. In other words, not only do the police have to be modeled explicitly, but the probabilities of their actions have to be *derived* in the same manner as the probabilities of the public: through rational calculations.

A Game-Theoretic Analysis of Crime

Table 3 presents the payoffs of the police considered as a rational player. In order to find the ranking of these payoffs, I will proceed by way of assumption, as I did for the public. Assume that the police prefer to enforce the law when it is violated and that due to enforcement costs, the police prefer not to enforce the law if it is not violated. Formally, these assumptions can be stated, $a_2 > b_2$ (A3′) and $d_2 > c_2$ (A4′).

If the police enforce the law, the public will stop violating it (A2′), if the public stops violating the law, the police will stop enforcing it (A3′), if the police stop enforcing the law, the public will violate it (A1′), if the public violates the law the police will enforce it (A4′), if the police enforce the law, then the public will stop violating it (A2′), and so on. In other words, the conditional probability that the police will enforce the law (given its violation) is equal to one, while the conditional probability that they will enforce the law (assuming it is not violated) is zero. Similar statements concerning conditional probabilities can also be made for the public. These statements indicate the fundamental criticism mentioned in the previous section: that actors maximize against each other and not against nature, so their acts are not independent.

In this game between the police and the public, no matter which combination of strategies results from the choices of the two players, one of them will have the incentive to modify the choice. In game-theoretic terms, the game presented in Table 3 has no pure strategy (Nash) equilibrium since the two players have no pure strategies that are optimal responses to each other. Therefore, the only equilibrium strategies that exist in this game are mixed strategies that are probability distributions over the set of pure strategies (Ordeshook 1986, chap. 3). In other words, each player will use a combination of the pure strategies so that when both apply a specific pair of strategies, neither will have an incentive to deviate from his or her mixture. Each player's strategy will be the optimal response to the other player, therefore, both players will retain these (equilibrium) strategies.

In terms of probabilities, the fundamental difference between the decision-theoretic and the game-theoretic approach is that probabilities are not given exogenously (as in the example of rain), but come as the derivative of rational calculations on the part of the actors: acts are interdependent and therefore the conditional probabilities of acts are different from the marginal probabilities.

Table 3. Payoffs in the Police-Public Game

Decision	Enforce	Not enforce
Speed	$a_1\ a_2$	$b_1\ b_2$
Not speed	$c_1\ c_2$	$d_1\ d_2$

Note: Each player has two strategies. Payoffs of the public: $c_1 > a_1$ and $b_1 > d_1$. Payoffs of the police: $a_2 > b_2$ and $d_2 > c_2$.

In order to compute equilibrium strategies we must (1) assign a probability p to the public that chooses to violate the law (and $1 - p$ to those who abide by it) and a probability q to the police who enforce the law (and $1 - q$ to those who do not enforce it) and (2) find a pair (p^*, q^*) of p and q with the quality that the best answer for the public (provided the police follow the mixed strategy, specified by q^*) is to mix its pure strategies using p^* and $1 - p^*$ as weights; while the best answer for the police (provided the public follows the mixed strategy, specified by p^*) is to mix their pure strategies using q^* and $1 - q^*$ as weights. The calculation of p^* and q^* gives (Luce and Raiffa 1957)

$$p^* = (d_2 - c_2)/(a_2 - b_2 + d_2 - c_2) \quad (1)$$

$$q^* = (b_1 - d_1)/(b_1 - d_1 + c_1 - a_1) \quad (2)$$

We can verify that the specified probabilities p^* and q^* are in the (0,1) open interval and are therefore acceptable as solutions because of assumptions 1′–4′. Moreover, since the game has no pure strategy equilibria, the mixed strategies specified by the probabilities p^* and q^* are the *unique* equilibrium strategies of the game.

This last proposition is very important.

It must be singled out and stated formally.

THEOREM 1. Under assumptions 1′–4′ the only equilibrium in the police-public game is in mixed strategies as specified by equations 1 and 2.

This specification of the problem enables us to study the impact of different policy measures on crime. For example, what happens if the legislator influenced by the arguments of economic analysis increases the penalty for a crime? The size of the penalty is represented in the model by the payoff a_1, which the public receive when they violate the law and the police enforce it. Examination of equations 1 and 2 indicates the following important and counterintuitive outcomes.

THEOREM 2. An increase in the penalty leaves the frequency of violation of the law at equilibrium (p^*) *unchanged.*

THEOREM 3. An increase in the penalty *decreases* the frequency that the police enforce the law at equilibrium (q^*).

The proof of these two theorems is straightforward: inspection of equation 1 indicates that p^* does not depend on a_1, while equation 2 indicates a monotonic relationship between a_1 and q^*. What is needed, however, is to stress the reason for the resulting discrepancy between conventional wisdom and decision theory on the one hand and game theory on the other: in game theory both players maximize simultaneously while playing against each other. Neither of them can be assimilated to a probability distribution over states of the world. While it is very plausible that when penalties increase, the public (in the short run) will reduce violations of the law, it is also plausible that once the police realize this change in criminal behavior they will modify their own strategy; that is, they will try to reduce the frequency of law enforcement. The public will then modify again, and then again, and the new equilibrium will be the one described by equations 1 and 2 in which the increase in the penalty has no impact on criminal behavior.

Other policy prescriptions stemming from a sociological approach to the problem of crime suggest the development of welfare policies so that crime will cease to be an attractive career (Tittle 1983). In terms of the game-theoretic model, such policies would represent an increase of c_1 or d_1 or both—the payoffs for not violating the law whether the police enforce it or not. Note that the logic of these policy prescriptions is very similar to the logic of the economic approach to crime: they reward virtuous behavior instead of punishing deviance. In both cases, however, the expected utility of crime relative to compliance with the law is expected to decrease.

Examination of equations 1 and 2 indicate the following consequences of increasing welfare policies:

THEOREM 4. An increase of either c_1 or d_1 (welfare measures), leaves the frequency of violation of the law at equilibrium (p^*) *unchanged.*

THEOREM 5. An increase of either c_1 or d_1 (welfare measures) *decreases* the frequency that the police will enforce the law at equilibrium (q^*).[3]

The intuition behind these theorems concerning the sociological approach to crime is exactly the same as before: although in the short run such measures will have the desired impact, this will lead the police to modify their strategy and enforce the law less frequently. The public, in turn, will increase the frequency with which they violate the law. Thus, the process will finally equilibrate at the strategies described by equations 1 and 2.

The conclusions of the game-theoretic analysis reported in theorems 1–5 are strongly counterintuitive. It is therefore only reasonable to question the logical and empirical foundations of the validity of the game-theoretic approach. I will begin with the logical evidence. There are several specifications that lead to the outcome of equations 1 and 2.

Rational players with complete information. The equilibrium of the public-police game is unique. Therefore, it can be shown to have all the desirable game-theoretic properties of stability. In particular, this Nash equilibrium can be shown to be perfect (Selten 1975), proper (Myerson 1978), sequential (Kreps and Wilson 1982), and stable (Kohlberg and Mertens 1986) (see Tsebelis 1987a). Therefore, there is good reason to believe that rational actors will choose the equilibrium strategies described by equations 1 and 2.

Rational players with incomplete information. Suppose now that both players are rational but that each one of them knows only his or her own payoffs and does not know the payoffs of the opponent. If each player knows that there is a random element in the opponent's payoffs, the frequency with which this player will choose each one of the pure strategies tends to the equilibrium described by equations 1 and 2 as uncertainty tends to zero.[4] A natural interpretation of this approach would be that different police agents with different payoffs interact with different members of the public with different payoffs and that the frequency of choice of different strategies tends to the outcome described by equations 1 and 2 as these differences tend to zero.

Evolutionary approach. The same equilibrium can be supported under much weaker assumptions of adaptive behavior by both players through an evolutionary approach (Tsebelis 1987a). In this case individual actors are supposed to fulfill not the extraordinary requirements of rational choice theory but those of myopic adaptive behavior, leading them to choose at any time the strategy that pays better. So if the interaction of the public with the police is modeled along the time dimension, if after each time period each member of the public compares the results of abiding with the law with violating it and selects the most profitable behavior and if the police do the same (compare whether it is better for them to enforce or not to enforce the law), the equilibrium of this game is given by equations 1 and 2.

Mixed approach. It can be shown also that in the more realistic situation in which one of the players is fully rational (the police in this case, since it has a central organization and discipline) while the other is myopic (the unorganized public), the same strategies prevail at equilibrium.[5] The reason is that if the police choose any frequency q higher than q^*, the public will stop violating the law altogether, and the police will want to reduce the frequency of law enforcement. And if the police choose any frequency lower than q^*, the public will violate the law always, which will make the police increase q. So any frequency different from q^* cannot be an equilibrium. The relevant technical concept in this case is the Stackelberg equilibrium. But the Stackelberg equilibrium has a wider use: it is applicable when the two players move sequentially instead of simultaneously. So equations 1 and 2 indicate the equilibrium strategies of the opponents even in the case of a sequential game. A sequence of moves is not the natural interpretation of the police-public game, but it is the most appropriate assumption in other similar games, such as economic sanctions between nations, where the target country moves first and decides whether to violate some rule, norm, or economic interest of the sender country, and the latter decides whether to or not to punish. In such sequential games, the equilibrium is described by equations 1 and 2.

The last two specifications indicate that the assumption that the public is a unified rational actor capable of strategic behavior is not required to support the equilibrium of equations 1 and 2. Indeed, in the last two cases, the public—or more precisely *each member* of the public—is assumed to maximize its own payoffs without any strategic considerations. The fact that the game leads to the same outcome with such diverse specifications as perfect rationality, adaptive behavior, and complete or incomplete information is an indication of the robustness of the equilibrium (equations 1 and 2).

The empirical evidence concerning the absence of impact of penalty on crime is indirect. Although there is an abundance of empirical studies on crime and the impact of the frequency of law enforcement has been established, the impact of the size of the penalty has not.[6] Some empirical studies find a negative correlation between frequency of crime and size of penalty while others find a positive correlation. The explanation for the latter may be that when the frequency of crime increases, the legal system is likely to become more strict. Nagin, reviewing the empirical literature on the deterrent effect of penalties on crime for the National Academy of Sciences, concludes, "Yet, despite the intensity of the research effort, the empirical evidence is still not sufficient for providing a rigorous confirmation of the existence of a deterrent effect. Perhaps more important, the evidence is woefully inadequate for providing a good estimate of the magnitude of whatever effect exists. . . . Any unequivocal policy conclusion is simply not supported by valid evidence" (1978, 135–36). This empirically based statement is quite similar to theorem 2, which was derived from the game-theoretic approach. So the absence of clear-cut evidence in favor of this particular aspect of the economic theory of crime is an indirect empirical indicator of the validity of the game-theoretic approach.

The logical and empirical evidence in favor of the game-theoretic approach indicates that both dominant approaches to crime prevention—the sociological and the economic—are partial and short-run in nature. They are both based on the assumption that modifying the incentive structure of a rational agent (modifying the payoffs) will affect that agent's behavior. Reasonable and plausible as this assumption may seem, it was shown not to withstand game-theoretic scrutiny.

To recapitulate, I considered the problem of traffic law violations not as a problem in decision theory but as a game between the police and the public. The superiority of the second approach over the first arose from the fact that the probability of law enforcement was not given exogenously (as in the rain example) but derived as a conclusion from the rationality assumption and from the payoffs of the players. Modifying the penalty had no impact on the frequency of criminal activity, as is commonly assumed. Instead, the frequency of law enforcement is affected.

How frequently can common sense conclusions and widely held beliefs—such as the deterrent effect of penalties—be shown false under closer game-theoretical scrutiny? How common is the Robinson Crusoe fallacy? I argue that such situations are very frequent.

The Abuse of Probability and Decision Theory

Do the counterintuitive results of the previous section require unusually strong or unrealistic assumptions? Are they an artifact of the 2×2 game?

I have already demonstrated that the equilibrium solution holds even if we relax the requirement of strong rationality and replace it with one-sided rationality, mutual myopic behavior, or conditions of incomplete information. However, the absence of an impact of penalty on crime may be due to the restricted strategy spaces of the two players. What happens if each player has more than two strategies?

Let us consider more serious crimes and assume that the criminals have the option to commit more than one crime and that the police have the option of dividing their law enforcement activities among the different crime activities. Construct a payoff matrix for this game similar to Table 3, in which each player has $n + 1$ options: the criminals have n different crimes and obedience to the law (which I will call the criminals' zero option); and the police have enforcement of the law for each one of these crimes and no enforcement (which I will call the police's zero option). This situation is more realistic than the simple 2×2 original game. Note that each player now can choose not only in a binary way (zero option or not) but has a choice between different ways of spending time as well as all the combinations among them (mixed strategies).

Studying such a game will provide information about the consequences of modifying the penalty of one type of criminal activity. However, I will not construct and solve such a game. Instead, I will indicate the reasons why such a modification of one or all the penalties has, in this general case, consequences similar to those of the 2×2 case. Such a modification has no impact upon the behavior of the criminals, but it influences the behavior of the police.

To calculate the Nash equilibria of this game one has to verify whether pure strategy equilibria exist. If the game has a pure strategy equilibrium, the players will choose their pure equilibrium strategies. It is unlikely that this $(n + 1) \times (n + 1)$ two-person game has one pure strategy equilibrium, because in this case this equilibrium would still be an equilibrium in the 2×2 game in which the available options for each player would be the particular crime and the zero option. I have demonstrated, however, that a pure strategy equilibrium in the 2×2 game is very unlikely.

Nonetheless, it is possible that the $(n + 1) \times (n + 1)$ game has several pure strategy equilibria, which are combinations of mutually optimal strategies. In this case, we would observe the criminals and the police coordinating their activities in order to achieve one of two things: (1) when the criminals violate one particular law, the police are always there to enforce it, or (2) the police are never there when a crime is committed. Again, the arguments of the second section indicate the unlikelihood of such a situation.

Thus, the only remaining case is that the $(n + 1) \times (n + 1)$ game has no pure strategy equilibria. Two theorems can be demonstrated in this case.

THEOREM 6. If a two-player game has no pure strategy equilibria, it has a unique mixed strategy equilibrium.

THEOREM 7. In a two-player game with no pure strategy equilibria, modification of the payoffs of one player will lead that player either to change the pure strategies that he or she mixed or to leave the mixed equilibrium strategy unchanged and modify the equilibrium strategy of the opponent.

The existence of such an equilibrium has been proven by Nash (1951). The uniqueness and theorem 7 follow from the method in which this mixed strategy equilibrium is computed. Each player must choose a strategy mixure that will make the opponent indifferent as to his or her own strategies. Therefore, each player has to choose the probabilities of his or her own mixed strategy in such a way that the expected utilities of the other player's strategies will be equal. These calculations lead to a linear system of n equations with n unknowns (the probabilities that each player will make use of the different strategies) in which the coefficients are the payoffs of the *other* player. This remark proves theorem 7, while the uniqueness outcome of theorem 6 follows from the linearity of the system.

Now that the mathematical robustness of the outcome has been established, I focus on another, more interesting question. What is the frequency of games like the general $(n + 1) \times (n + 1)$ game I have just described? I will demonstrate that cases of the Robinson Crusoe fallacy—that is, situations in which decision theory is (mis)used in the place of game theory and leads to incorrect results—are very frequent, by providing some examples of such situations. Finally, I will give a set of sufficient conditions for such a misuse to occur.

Consider the case of regulated industries. Their situation is similar to the public of our 2×2 game while the regulating agency plays the role of the police in our 2×2 game. Industries prefer to abide by the law if they are going to be investigated and prefer to violate it when they know they will not be caught.[7] Agencies prefer to enforce the law when it is violated and to relax when industries conform to regulations. Again, most of the time there is no pure strategy equilibrium. Therefore, the unique existing equilibrium is in mixed strategies with the properties we have discussed: changes in fines or penalties do not affect the behavior of firms at equilibrium. This result is in sharp contrast with the economic literature on regulation.[8]

Consider the problem of international economic sanctions and the big dispute concerning their effectiveness. There seems to be a consensus among scholars that sanctions do not work.[9] Despite scholarly opinion, economic sanctions are applied with increased frequency over time.[10] If economic sanctions work, why do scholars suggest they do not? And if they are not effective, why do policymakers insist upon imposing them? The puzzle can be solved in terms of our simple model. There are two possible cases: either the penalty imposes on the target country costs more serious than the benefits of deviant behavior, or it does not. In the first case, there is no pure strategy Nash equilibrium, and the model developed in the second part applies, making the behavior of the target country independent of the size of the penalty. In the second case, there is a pure strategy Nash equilibrium, and deviant behavior should be observed all the time and no penalty imposed.[11]

Consider problems of hierarchies. Can the higher level be sure that lower levels will conform to given instructions? Should it increase rewards for observation of the internal rules of the organization? Should it punish deviant cases in an exemplary way? Should it increase monitoring frequency? In other words, which policy works better—the carrot or the stick—and how often should it be applied? Again, if the payoffs of the two players are such that there is no pure strategy Nash equilibrium, the size of the rewards or the penalties will have no impact on the behavior of subordinates at equilibrium. However, higher rewards and penalties will reduce the required monitoring frequency. Therefore, if the higher level of hierarchies can design the rules of the game (rewards and punishments), they will try to increase both not in order to reduce deviant behavior but to reduce the required monitoring frequency. The question of endogenous changes of the rules can be dealt with in terms of a two-stage game: in the first stage the higher level sets the rules (the payoffs), and in the second stage the actual game is played with simultaneous moves by higher- and lower-level agents. Given that the equilibrium in the second stage can be calculated exactly as in the game between the police and the public, one can work the solution of the game backwards, substitute the equilibrium payoffs for the actual games, and then choose between different games according to some utility function. For example, the higher levels of a hierarchy may want to consider a trade-off between monitoring frequency and levels of anxiety generated by high penalties. The higher levels of a hierarchy are thus able to maximize their utility no matter what the specific utility function is and what trade-offs the utility function establishes.

Finally, consider the problem of legislative oversight. It has been argued that Congress prefers the "fire alarm" system, where citizens or groups monitor the executive branch, as opposed to the "police patrol" system, which requires more active monitoring, because the former system is more cost-efficient (McCubbins and Schwartz 1984). However, empirical findings indicate that fire alarms do not constitute the majority of cases of legislative oversight (Aberbach 1987). The police-public game can help investigate this discrepancy. Indeed, the model can include three different strategies of the law enforcing agency: fire alarms, police patrols, and no action. Explicit use of the relevant payoffs will indicate whether there is a pure strategy equilibrium, as McCubbins and Schwartz claim, or whether there is a mixed strategy equilibrium, as Aberbach's empirical findings seem to indicate. Even further, the model will help discriminate between the necessary and sufficient conditions for each case.

All the above examples have to do with questions of authority. What is the appropriate strategy to induce citizens, firms, countries, subordinates, bureaucracies, and so on to conform to a set of rules? The results are quite different from the conventional wisdom. Conformity to the rules cannot be achieved (at equilibrium) either by the carrot or by the stick. The impact of such measures will be the reduction of monitoring frequencies. If the frequency of deviant behavior is the target, the payoffs to be modified are those of the monitoring agency.

What explains these discrepancies between widely held beliefs and closer game-theoretic scrutiny? At the heart of the matter lies the concept of probability. The very concept of probability in the social sciences involves the mixture of two completely different concepts, which I will call partition frequencies and mixed strategies.

Partition frequencies refers to some unknown or unknowable desirable element of information that enables the decision maker (or the player) to make the correct choice in a particular situation. Therefore, this information is a public good. An actor who is willing to provide it will provide substantial benefits not only to him- or herself but to all other actors involved in the same or a similar situation. *Mixed strategies*, on the contrary, are probabilities generated by rational players in order to disseminate information about the choice of their strategies. In fact, the opponent who acquires information about the choice of strategies of a player will have a decisive advantage over this player. Therefore, information about mixed strategies is a private good that cannot be acquired from the opponent. The concept of a mixed strategy Nash equilibrium is designed to prevent the opponent from acquiring information that would put him or her in a strategically advantageous position. Thus, the second use of the word *probability* is exactly the opposite of the first.

The distinction between partition frequencies and mixed strategies is meaningful only in the social sciences. The Robinson Crusoe fallacy is the substitution of partition frequencies for mixed strategies, and this is the culprit causing the confusion and wrong conclusions in all the previous examples.

Consider the meaning of the word *probability* in various kinds of explanations of social phenomena. Elster (1983) claims that there are three possible types of explanation in the social sciences:

causal, functional, and intentional. He immediately proceeds one step further by questioning the validity of functional explanations in the social sciences.[12] Regardless of their validity, functional explanations have been shown to be shorthand expressions for causal arguments (Hempel 1965, chap. 11). Therefore, we are left with two different generic explanations: causal and intentional. The difference is that causal explanations explain phenomena by some set of antecedent conditions, while intentional explanations make use of consequences and conscious decision makers (or players).

I will focus on intentional explanations because they are the only kind of explanations in which the distinction between partition frequencies and mixed strategies is meaningful. Intentional explanations are divided into decision-theoretic and game-theoretic species. The difference between these two categories is that decision theory situations use what Elster calls "parametric rationality," meaning that the decision maker can anticipate (exactly in decision theory under certainty, probabilistically in decision theory under risk) the situation that will pertain. In game theory, however, all players respond to each others' moves optimally, adapting to a changing situation in which the changes are caused in part by the players' own moves.

In decision theory, if one of the parameters of the situation changes, this change may create a different choice situation and therefore a different outcome. Thus, the probabilities of different choices correspond to the probabilities of different choice situations. Probabilities in decision-theoretic situations are, therefore, partition frequencies.

I will subdivide game-theoretic problems into three different classes: (1) dominance solvable games and games with unique, pure strategy equilibria, (2) games with multiple, pure strategy equilibria, and (3) games without pure strategy equilibria (that have one, mixed strategy equilibrium) (Nash 1951).

In dominance solvable games, each one of the players can eliminate his or her dominated strategies; when all players successively follow this procedure several times, if necessary, there remains only one acceptable strategy for each player (Moulin 1981). Similarly, if assuming perfectly rational players (more precisely, if perfect rationality is common knowledge) in games with a unique pure strategy equilibrium, one expects players to choose the strategies that correspond to this unique equilibrium because they know that these are the only mutually optimal responses.[13] In such games the outcome is as predictable as the apple falling on Newton's head. If a mixture of outcomes is observed, the logical inference to be made is that there is a mixture of games.[14] Probabilities in such cases are partition frequencies.

The case of multiple equilibria is more complicated because a unique set of mutually optimal strategies does not exist. Here, the players face a coordination problem: each one may have different preferences about which is the most desirable equilibrium. However, each prefers to choose the appropriate equilibrium strategy, as all the others do. As a result, there is created an incentive for all of them to sort the different kinds of equilibria by using, for example, a common signal. If such a signal exists, it will be used and the situation will be simplified into a decision problem of the previous category. The presence of the signal will indicate the choice of a specific set of strategies and the frequency of the signal will indicate the probability of choice of the particular equilibrium. I do not know of any account of such games in which the choice of equilibria is made endogenously, without the use of some external signal.

In the case of no pure strategy equilibrium, each player has a unique mixed

Table 4. Different Problems and the Relevant Concept of Probability

Nature of Explanation	Source of Probabilities	Kind of Probabilities
Causal	mixture of causes	partition frequencies
Intentional		
Decisions	mixture of situations	partition frequencies
Games with more than one equilibrium	different strategy combinations	partition frequencies
Games with one pure strategy equilibrium	mixture of games	partition frequencies
Games with one mixed strategy equilibrium	mixed strategies	mixed strategies

equilibrium strategy (a probability distribution over his or her pure strategies) that is the optimal response to the mixed strategy choices of the others. Deviation from these strategies will provoke an infinite cycle of deviations in which each player either will be punished and made worse off or find an opportunity to deviate further. Therefore, each player has an incentive to prevent the opponent(s) from guessing how that player will choose. This can be achieved by randomizing the strategies. In the absence of a pure strategy equilibrium, therefore, each player will want to hide his or her choice. Probabilities in this case express an element of strategic choice, not an "objective" situation that would have been a means of exploitation of some players by others.

Table 4 summarizes the argument. Causal explanations have been included for reasons of completeness. Without entering into the important epistemological debate between determinists and indeterminists,[15] it can be said that whenever the concept of probabilistic outcomes occurs in causal explanations, it refers to the mixture of different causes and therefore expresses partition frequencies.

To summarize, there are two different ways the word *probability* can be used. I have called them partition frequencies and mixed strategies. Mixed strategies do not occur in causal arguments, in decision-theoretic situations, or even in games with pure strategy equilibria. They occur only in games with mixed strategy equilibria so that a player will not be outguessed and bested by the opponent. I have provided several examples of such cases and think that they can be multiplied: any situation in which inside information is valuable and "intelligence" is used to collect it is prima facie a candidate to be modeled as a game without a pure strategy equilibrium in which probabilities are in fact mixed strategies and not partition frequencies.

Whenever such a substitution of partition frequencies for mixed strategies occurs, we are led to wrong beliefs. We expect a change in the payoffs of one player to influence that player's behavior, when in reality such a modification gets completely absorbed by a change in the strategy of the opponent.

Notes

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1. The "sure thing" principle formally states that if two acts are such that the first is preferred over the second, *given a state of the world*, but have the same consequences otherwise, the first is *unconditionally* preferred over the second. This axiom does not hold if the states of the world are dependent on the acts chosen.

2. The reader may object that these statements assume both the public and the police to be unified actors. In this case it is true that what the police does depends on the action of the public. However—the argument may go—the behavior of the police does not depend on the actions of any individual member of the public. For the time being assume the public as a unified actor. I will address this objection in more detail when I present alternative specifications of the model leading to the same outcome.

3. It is easy to verify that the first derivatives of equation 2 with respect to d_1 and c_1 are negative. A more intuitive way to verify theorem 5 is to observe that a decrease in the numerator and the denominator of a fraction by the same amount decreases the fraction.

4. For a model with incomplete information and the proof of this proposition, see Tsebelis 1987c. For the general proof that a mixed strategy equilibrium can be supported by incomplete information assumptions, see Harsanyi 1973.

5. See Tsebelis 1987a. I am grateful to Michael Wallerstein for indicating this to me.

6. See Tsebelis 1987a for references to the empirical literature.

7. This is not the only possible assumption. One can assume, alternatively, that they will violate the law anyway, taking their chances in the subsequent litigation process. In this case a pure strategy equilibrium exists (*violate* for the industry and presumably *enforce* for the agency).

8. See Tsebelis 1987b for an extended development of the regulation game, as well as for a review of the economic literature.

9. See the review by Olson (1979).

10. See the exhaustive empirical study by Hufbauer and Schott (1985).

11. See the exhaustive empirical study by Hufbauer and Schott (1985).

12. See also Elster 1985. For a different opinion, see Stinchcombe 1968.

13. The same argument applies if there are multiple (Nash) equilibria all but one of which can be eliminated as unstable. In this case some refinement of the Nash equilibrium concept like perfect (Selten 1975), proper (Myerson 1978), or sequential (Kreps and Wilson 1982), can help eliminate "unreasonable" equilibria. For a full account see Damme 1983.

14. Or, of course, it might be that the assumption of common knowledge of rationality is wrong. In this case, the game can be modeled as a game of incomplete information (Kreps and Wilson 1982) or imperfect information (Trockel 1986). These cases do not present a unique equilibrium and therefore belong to the next category.

15. For a deterministic view of causality, see Bunge 1979. For an indeterministic view, see Popper 1965.

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George Tsebelis is Assistant Professor of Political Science, University of California, Los Angeles, CA 90024.

A DIRECTIONAL THEORY OF ISSUE VOTING

GEORGE RABINOWITZ
STUART ELAINE MACDONALD
University of North Carolina
Chapel Hill

From Stokes's (1963) early critique on, it has been clear to empirical researchers that the traditional spatial theory of elections is seriously flawed. Yet fully a quarter century later, that theory remains the dominant paradigm for understanding mass-elite linkage in politics. We present an alternative spatial theory of elections that we argue has greater empirical verisimilitude.

Based on the ideas of symbolic politics, the directional theory assumes that most people have a diffuse preference for a certain direction *of policy-making and that people vary in the* intensity *with which they hold those preferences. We test the two competing theories at the individual level with National Election Study data and find the directional theory more strongly supported than the traditional spatial theory. We then develop the implications of the directional theory for candidate behavior and assess the predictions in light of evidence from the U.S. Congress.*

The spatial theory of electoral choice has had enormous influence on how elections are perceived and studied. Popularized by Downs (1957) and more fully formalized over the next three decades (e.g., Davis and Hinich 1966; Davis, Hinich, and Ordeshook 1970; Enelow and Hinich 1982, 1984; Hinich and Pollard 1981), the model is a central component of both formal and behavioral political science. The popularity of the spatial model is not surprising. The essential model was developed independently by a statistician interested in explaining market locations (Hotelling 1929) and a psychologist interested in individual choice behavior (Coombs 1950). It has been further developed and analyzed by political economists interested in placing the dynamic of electoral competition in a rational choice framework.[1] No other formal paradigm has had such wide use or such great impact on how people think about politics.

The key ideas underlying the spatial model are that (1) each voter can be represented by a point in some hypothetical space such that the point reflects the person's ideal set of policies, (2) the policy position of each candidate can be represented by a point in the same space, and (3) a voter chooses the candidate whose policy position is closest to his or her own. Models of this genre are called proximity models, since preference follows directly from "closeness."

The spatial model has undergone serious criticism, the most systematic being that of Stokes (1963). While the model has been extended to overcome several elements of the Stokes critique, it has largely failed to take account of his "ordered dimensions" criticism, which questioned the assumption that voters perceive a set

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of ordered alternatives on the relevant issues (dimensions) of the campaign. Based on an empirical analysis of preference spaces, Rabinowitz (1978) concluded that the conventional spatial paradigm of issue impact was incorrect. He argued that issues operate in a "dispositional" or diffuse way. A major thrust of both these critiques was that the vast majority of the public did not see issues in the sharp positional fashion that the traditional theory assumes. The view that issues are perceived rather diffusely is also consonant with the symbolic politics concept of issue impact. We will build a theory of issue voting that is more consistent with the way individuals make judgments on the basis of political issues.

Issue Voting and the Cognitive Model

To question the adequacy of the spatial model from a cognitive process perspective may seem surprising. After all, the spatial choice model applied to elections is but an example of a general preferential choice model widely used in psychology (Coombs 1964). As a cognitive model in psychology, however, it has had its greatest payoff in situations where the structuring is very well defined, such as the subjective assessment of the perfect shade of gray. The relationship between issue preferences and candidate evaluation has far less direct structure than do the relationships in the classic applications in psychology. Thus, while the paradigm is surely powerful as a model of cognitive processing, it may *not* be the most appropriate model for relating political issues to candidate evaluation.

A useful contrast is that between the theory undergirding symbolic politics and that of the traditional spatial model. The key tenet of symbolic politics is that for issues (or other political cues) to have impact, they must evoke emotions and sentiments rather than simple objective appraisal of information (Edelman 1964; Sears, Hensler, and Speer 1979). According to the theory an issue conveys a symbol—for instance, race, health care, or taxation—that has the potential to trigger a set of associations based on prior experiences. These associations often generate emotional responses that make the issue politically meaningful.

If we attempt to represent this type of symbolic response in a formal way, we can imagine two qualities that are evoked by the symbol. First, there is a *direction* to the response. Does the person feel favorable or unfavorable toward the symbol? Second, there is a magnitude or an *intensity* to the response. How strongly does the person feel about the issue? Does it conjure up strong feelings with high emotional content or weak feelings with little emotional content? Issues that people react to in a symbolic way would thus be characterized by two components: (1) direction and (2) intensity. Symbolic politics does not lend itself well to the systematic consideration of an ordered set of alternatives that characterizes the traditional spatial model. Symbolic politics is by its nature associated with diffuse and emotionally laden reactions to issues. To the extent that issues are symbolic, the impact of issues should be modeled in terms of direction and intensity.

But even if we assume a great deal of issue processing is not symbolic in nature, there are reasons to favor a *directional* (direction-intensity) paradigm for understanding mass cognition of issues. What is critical to the directional perspective is that the basic issue is conceptualized in a diffuse fashion rather than in terms of a set of specific policy alternatives. Empirical analyses of mass publics have stressed that the vast preponderance of citizens operate with low levels of information (see, for example, Campbell et al. 1960; Hamill, Lodge, and Blake 1985). Based on findings accumulated over the last three decades, it is virtually inconceivable that the preferences for policy among the mass public go beyond a diffuse sense of direction.

Institutional factors in two-party systems also favor the directional model. In two-party systems voters receive cues that favor one direction of policy-making or another. The voter is thus encouraged to think in terms of alternate directions for policy rather than in terms of an ordered set of alternatives.

Questionnaire Design

A useful starting point for a discussion of issue processing is to examine how practicing political scientists interested in mass behavior have presented issues to people on surveys. The design of issue questions reflects how survey researchers—either intuitively or through practical experience—feel people understand and use issues in making political judgments. While there is no guarantee that these professional instincts are correct, they are of value in assessing the face validity of any model of issue voting.

The issue format that was most widely used in the early studies of electoral behavior was the agree-disagree format. By 1956 this was the dominant question style in the Michigan-based national election studies. The essential format was to state a position—for example, "The government ought to help people get doctors and hospital care at low cost"—and then to ask respondents whether they strongly agreed, agreed, were not sure, disagreed, or strongly disagreed with the statement. It is interesting to observe that this format was designed to elicit the *direction* of the preference. The respondents were asked to place themselves on one side or the other of the issue or simply to declare neutrality. By indicating whether they strongly agreed or disagreed, they were asked to indicate their *intensity*.

There have been two substantial changes in the design of issue questions. The first was to move away from the agree-disagree format in favor of stating two opposing sides of an issue. This change was made to minimize the tendency of respondents to yea-say, that is, to agree just to be agreeable. The second change was explicitly to introduce the concept of a scale to the question. This format change was introduced in 1968 by Page and Brody and extended to most of the standard issue questions in 1972. The format change was proposed with the specific intent to accommodate the spatial-rational choice paradigm and to allow for a more positional response.

Under this format respondents are shown a card on which a line is drawn with the endpoints labeled at positions 1 and 7 and asked to place themselves and candidates on the seven-point scale. This format corresponds very nicely to the requirements of the traditional spatial model. Respondents can place themselves anywhere along the seven-point scale, and researchers understand the placement to indicate the most desired policy position of the respondent. The format of the question, however, does not present an ordered set of alternatives. Rather, it poses two alternative positions on the issue. Of relevance to our argument is the fact that the form of the question permits directional thinking. Respondents thinking in directional terms would have little difficulty responding to this question. They would simply choose their preferred policy direction and how intensely they favored it.

The distinction between the two types of responses is significant. In the classic spatial formulation, the response indicates preferred *position* along a continuum of policy alternatives. There is no necessary implication with respect to intensity. Those in the center may be just as intense as those on the extremes. In the directional formulation, the extent to which a respondent is on one side or the other of the center conveys *intensity* rather than position on a scale. In the 1980 election study an attempt was made to measure both scale position and intensity by including a separate salience question following the placement question. This additional salience question proved to be of little value in empirical analysis (Niemi and Bartels 1985). Indeed, the use of personally assessed importance produced a slightly worse fit than the simple model in which the issues were weighted equally.[2] This finding is consistent with the view that the basic issue question is already interpreted by respondents in direction-intensity terms.

Aside from the efforts of the National Election Study, the most systematic attempt to innovate measures of issue attitudes has been the work of Lodge and Tursky and their associates (see, e.g., Lodge and Tursky 1981). Here again it is useful to see how these researchers designed issue questions. Their innovation was to introduce psychophysical measurement instead of the categorical measurement strategy of the NES studies. Rather than the fixed seven points of the NES issue scales, they introduced a method allowing an indeterminant range. The initial cue is very much like that of the NES issue questions, stating two positions as well as a neutral position. This is followed by a line-drawing task for which the exact wording is "Now, draw a response line to say how much you favor [policy alternative]" (Lodge and Tursky 1981, 408). "How much you favor" is a pure intensity measure; there is no hint of position in it.

Our interest in reviewing how issue questions have been posed to respondents is to understand the intuitive model that researchers have of issue processing in the mass public. Our brief review indicates that the general style of the questions has been of the directional variety. The use of positional questions has occurred because the classic spatial paradigm is so important to the discipline's understanding of the role of issues in elections. Nevertheless, even positional questions have usually been cast in a form amenable to direction-intensity thinking. Of course, all one can say is that questionnaire design has been consistent with the directional perspective. That does not constitute proof that this model is more appropriate for understanding individual issue processing than the proximity model. The mathematics underlying the two alternative models is different, however, and will provide the basis for a rigorous comparison between them.

Formalizing the Directional Model

The formalization of a model based on the directional perspective is fairly straightforward. The model calculates the impact of an issue on an individual as jointly determined by the directional compatibility of the individual and the candidate (or party) and the intensity levels of the individual and the candidate. We can break the process down into two steps. First, we must know whether the individual and candidate are in agreement about the direction public policy should take. Are they on the same side or opposite sides of the issue? If they are on the same side of the issue, the affect associated with the issue will be positive; if they are on opposite sides, the affect associated with the issue will be negative. Next, we must know how intense the individual and the candidate are with regard to the issue. How much emotion is associated with it for the individual, and how much does the candidate stir feelings on the issue? The magnitude of the impact of the issue is determined by an interaction of these two intensity levels. Empirically, the intensity level is determined by the extent to which the voter or the candidate is on one or the other side of the neutral point.

The formula to calculate the directional effect of an issue is

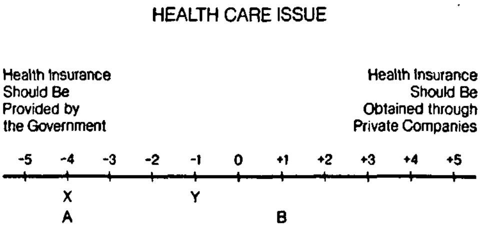
(candidate location − neutral point) ×
(voter location − neutral point).

Notice that if both voter and candidate are on the same side of the neutral point, the sign of the product will be positive; if they are on different sides, the sign will be negative. Notice, too, that the magnitude is an interactive function of the intensity level of both individual and candidate. If either the voter is neutral (has no emotional reaction to the issue) or the candidate is neutral (provides no stimulation with regard to the issue), the net impact of the issue will be zero. As the voter and the candidate become more intense, the directional effect is amplified. Of course, it is possible for a candidate to be so intense as to become an unacceptable "extremist." We will consider that possibility later, but for now it need not concern us.

Let us consider an example in which we have two voters and two candidates. One voter, whom we will label X, is strongly in favor of government-provided health care. The other voter, whom we will label Y, is only slightly in favor of government health care, not caring much one way or the other about the issue. Now consider two candidates: A is strongly in favor of government-provided health care, placing great emphasis on the issue. B is slightly opposed to government health care, placing very little emphasis on the issue in the campaign. We can represent the positions of the four actors on an 11-point scale and mark the neutral point as 0 to put the scale values in deviation form (Figure 1).

Both the directional and proximity theories predict that X will strongly support A on this issue. According to proximity theory, X and A are located at the same point. Thus they are 0 distance apart. According to directional theory, they are both on the same side and far from the neutral point. Hence, the calculation will be −4 × −4 = 16, the high positive number indicating strong affect.

Figure 1. Health Care Issue Scale to Illustrate Utility Calculations Using the Proximity and Directional Models


HEALTH CARE ISSUE
Health Insurance Should Be Provided by the Government
Health Insurance Should Be Obtained through Private Companies
-5
-4
-3
-2
-1
0
+1
+2
+3
+4
+5
X
Y
A
B

Note: A and B represent candidates; X and Y represent voters.

The directional and proximity theories predict different behavior from voter Y. In a proximity sense, Y is closer to B (2 units) than to A (3 units). But in a directional sense, that is not the case: Y is on the same side of the issue as A and on the opposite side from B. Hence, the theory predicts that Y will like A better than B. The values in this case will be −1 × −4 = 4 with respect to candidate A, and −1 × 1 = −1 with respect to candidate B. While both values are small in magnitude, the positive assessment with regard to this issue is for A, and the negative assessment is for B.

If we think in symbolic politics terms, the directional prediction makes sense. The voter who prefers one side of a debate to the other but cares little about the issue would not generally be expected to support the candidate who favors the opposite side and says little. To the extent that the symbol stimulates Y at all, it draws Y toward A and away from B. The effect of saying little about the issue, as B does, is to reduce the role of that issue in making judgments about B. It does not endear B to a person who has little interest in the issue.

In this same vein, we could imagine a candidate C located at the exact same place as Y, indicating that C weakly favored government-sponsored health care and placed little emphasis on the issue. In this case the directional model would calculate a −1 × −1 = 1 value between voter Y and candidate C. This is lower than the 4 calculated for candidate A. Thus, according to the directional theory, voter Y would evaluate candidate A more favorably than candidate C because of A's greater emphasis on the health care symbol. The logic of this result is similar to that for candidate B. The fact that C is located at the same place as Y does not endear C to Y. It simply means that Y is fairly indifferent about the policy, and C says little about it in the campaign. Indeed, we would expect Y to be relatively unaware of C's position, since Y is not particularly alert to cues emanating from candidates with regard to the health care issue. Candidate A, however, provides a clear enough cue that Y is aware of the direction of A's stand and they agree. This contrasts dramatically with traditional spatial theory in which C would represent the ideal candidate for voter Y.

If health care were the only issue in the campaign and this were a pure issue vote, then directional theory states that Y should weakly favor candidate A. If there were many issues in the campaign and candidates were restricted in the number of issues they could emphasize, then, of course, health care would be a poor choice for encouraging support from voter Y, since for Y the symbol has only limited impact. Similarly, if nonissue factors were important in the campaign, then the affect advantage that would accrue on the basis of the health care issue for a voter like Y would be minimal. In such a case other factors would be likely to determine vote choice.

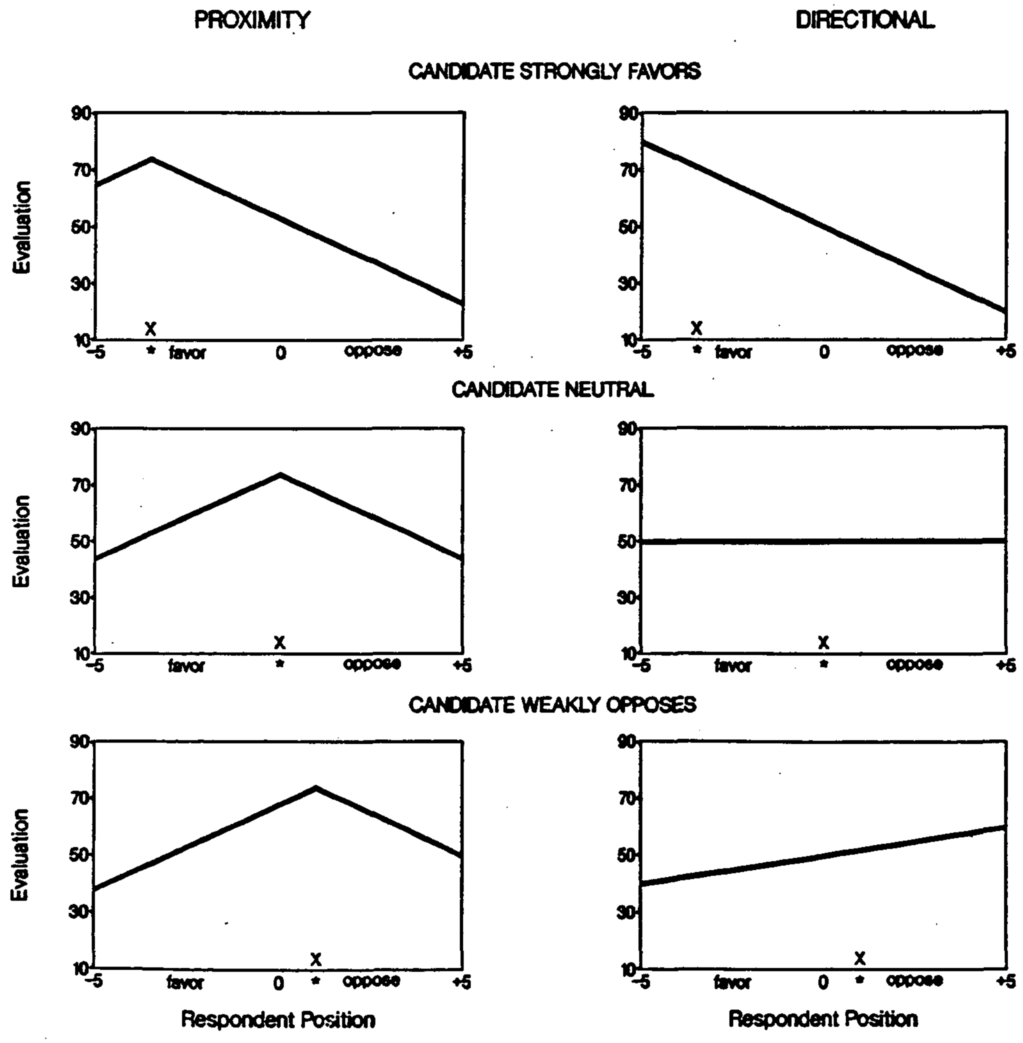
The general relationships between individual issue position and candidate evaluation in the two models are displayed in Figure 2. The graphs are shown for three candidates: one who strongly favors government health care, one who is neutral, and one who weakly opposes it. In each graph the position of the candidate is marked with a * and a letter. Implicit in the location of each candidate at a single point is the assumption that all voters are reacting to the same candidate stimulus. While this is a common feature of both models, we draw attention to it because of its theoretical importance.

The left side of Figure 2 shows the single-peaked graphs of the proximity model. Traditional spatial theory predicts that support will peak at the candidate's position and will decline as voters are located at an increasing distance from the candidate. In contrast, the relationship between issue position and affect is monotonic in the directional model (right side of Figure 2). The strongest support for the candidate comes at one extreme and the strongest antagonism comes at the other extreme, except for the neutral candidate who receives identical evaluations from everyone. Notice that the function of lower intensity is not to change the *shape* of the support curve but rather to *decrease its slope.* Relatively moderate candidates are less likely to be either "loved" or "hated" for the issue positions they advocate but otherwise tend to garner support from the same individuals as do more intense advocates.

This example is based on only one issue, but the direction-intensity idea readily generalizes to a multiissue situation. The more intense a candidate is on an issue, the more the candidate generates intense support or opposition with regard to that issue. By taking clear, strong stands, candidates can make an issue central to judgments about themselves. At the same time candidates who can successfully evade an issue are able to make that issue far less relevant for judgments about themselves. Thus, in a multiissue election candidates are likely to be intense on issues that benefit them and silent on issues that are potentially damaging.

This conceptualization of the role of candidates contrasts with that of traditional spatial theory. While the traditional model makes complex cognitive demands on individuals in the mass public, it places little control of issue emphasis in the hands of candidates. In the traditional model candidates are conceived to take a stand on every issue of relevance to any voter in the election, and issues must identically influence the evaluation of all competing candidates. But this does not accord with reality. In the course of elec-

Figure 2. The Theoretical Relationship between Respondent Issue Position and Candidate Evaluation in the Proximity and Directional Models



tion campaigns candidates actively try to manipulate the criteria used to evaluate themselves, and often those used to evaluate their opponents as well. In this regard the directional model is more consonant with empirical analyses, which suggest that candidates competing in the same election are frequently judged by quite different criteria (Gant 1983; Macdonald et al. 1988; Mauser 1983).

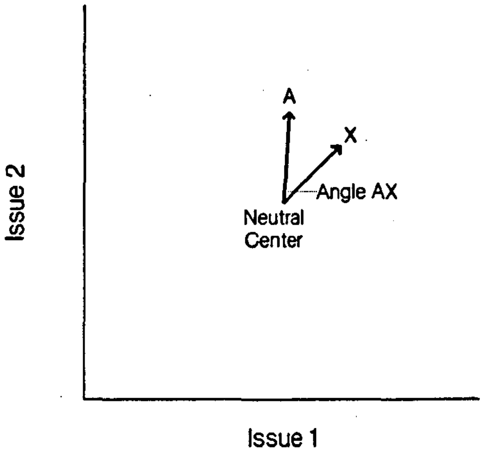
Spatial Representation of the Directional Model

We are now in a position to consider more generally the mathematics of a direction-intensity space. The formula changes from the unidimensional case simply by adding a summation sign over the various issues that underlie the space. Thus the net issue effect is

$$\Sigma\,(\text{candidate location}_i - \text{neutral point}_i) \times (\text{voter location}_i - \text{neutral point}_i),$$

where *i* indexes the various issues of relevance in the campaign. In mathematics, the value calculated according to the above formula is called a *scalar product.*

Figure 3. Illustration of the Scalar Product



The scalar product has a spatial representation that is useful to introduce. This is best presented through an example like that in Figure 3. In this figure there are two points, one point representing a voter labeled X and the other representing a candidate labeled A. According to the theory, spatial location reflects a direction and an intensity. To indicate the direction of the voter's policy preferences, we draw an arrow (vector) from the neutral point (the origin) to point X. The length of the arrow from the origin to point X represents the voter's intensity. Similarly, the direction of the A arrow represents the candidate's direction, and the length represents the candidate's intensity.

An alternate formula reflecting the spatial character of the scalar product is

$$|A|\ |X|\ \cos AX$$

where $|A|$ represents the length of vector A, $|X|$ is the length of vector X, and cos AX is the cosine of the angle formed by vectors A and X.[3] The cosine measures the similarity of the policy preferences of the voter and the candidate. It takes on a value of one if the angle is zero degrees and the two vectors are oriented in exactly the same direction and a value of minus one if the vectors are oriented in exactly the opposite directions. If the vectors are at right angles to each other, the cosine is zero. The more similar the direction of policy preferences, the more positive the scalar product; the more different the direction, the more negative the scalar product. The more intense the voter and the more intense the candidate, the more the directional effect is amplified.[4]

The unidimensional example we discussed earlier fits this spatial representation perfectly but in a somewhat trivial way. In the unidimensional example the length of the vector is equal to the absolute value of the scale location, and the cosine of the angle between candidate and voter is plus one if both vectors are on the same side of the neutral point and minus one if the two vectors are on opposite sides.

The evaluation of a candidate is thus measured by the compatibility of policy preferences (directional similarity) between voter and candidate and the intensity level of voter and candidate. High intensity raises the stakes. If a voter is directionally compatible with a candidate, increasing candidate intensity makes the voter like the candidate more. If a voter is directionally *in*compatible, intensity makes the voter like the candidate less. A location at the exact center of the space conveys complete neutrality. At the center the vector has no length, indicating no intensity and no direction. A candidate at the exact center of the space should be equally liked and disliked by everyone, since such a candidate would stimulate no emotions, either favorable or unfavorable.

This is a substantially different theory of how issues influence voter affect for a candidate. According to traditional spatial theory, distance from a candidate determines the affect of the voter toward that candidate. The directional theory proposes that a combination of directional compatibility and intensity determines affect. Since the predictions of the two models differ, it is possible to place one against the other in an empirical test to see which gives a better fit to the available data.

An Empirical Test at the Individual Level

The most natural test of the two models is to compare how effectively each predicts candidate evaluation on the basis of issues. To do this, we shall use all of the candidates for whom sufficient data exist in the presidential election surveys conducted by the National Election Study. Affect will be measured by the hundred-point feeling thermometers; issue positions will be assessed on the seven-point issue scales.[5] Respondents were asked to locate themselves and, in 1972, Nixon, McGovern, and Wallace; in 1976 Ford and Carter; in 1980 Carter, Reagan, Anderson, and Kennedy; and in 1984 Reagan and Mondale. This allows for 11 comparisons between the two theories.

For each respondent we will compute the average distance from each candidate over the set of seven-point issue items on which the respondent locates himself or herself and the candidate. Similarly, we will calculate the scalar product for each respondent-candidate pair.[6] In assessing the placement of candidates on the seven-point issue scales, we will follow Markus (1982) in locating the candidates at the mean of their perceived positions. This strategy is consistent with the assumptions of both theories, which require a single location for each candidate on each issue.[7] Respondents will be represented by their self-placement on the issues. In each case we will compare the explanatory power of the distance measure with that of the scalar product.

The comparisons will be made in two ways: (1) using the issues alone to predict affect and (2) embedding the issues in a general model that includes partisanship, race, and region. This will provide both an upper and lower bound for the explanatory power of the two competing models. If the distances offer more explanatory power than the scalar products, that will provide support for the conventional view that the proximity of voter to candidate determines the utility of a candidate for a voter. If, however, the scalar products offer more explanatory power, the evidence will point toward the directional model.

In making the comparisons we should not expect to see large differences. Both the proximity model and the directional model use identical information to relate issue position to candidate preference.

The differences between the models are of great theoretical importance, but the two measures are sufficiently collinear that large differences in predictive capacity are unlikely. The traditional model has guided electoral research for several decades and has been incorporated in virtually every analysis of individual voting decisions that has appeared in the leading scholarly journals since 1972 (see, for example, Jackson 1975; Markus 1982; Markus and Converse 1979; Miller et al. 1976; Page and Jones 1979). Hence, it will be noteworthy if any systematic differences appear that favor the directional model.

Results

Table 1 shows the results for the 1984 election and illustrates the regression analyses. In the table we show the regres-

Table 1. A Comparison of Proximity and Directional Models of Issues in Predicting Candidate Evaluation, 1984 National Election Study

Models	Parameter Estimate	Standard Error	Standardized Parameter Estimate	Adjusted R²	% Variance Explained by Issues[a]	
					Upper Bound	Lower Bound
Reagan (number of cases = 2,080)						
Proximity model						
- Euclidean distance	9.02	.67	.24	.451	19.3	4.8
Party	7.08	.25	.51			
Race [black]	-11.35	1.67	-.12			
Region [South]	7.96	1.14	.12			
Constant	57.99	1.69	—			
Directional model						
Scalar product	7.98	.47	.30	.474	26.2	7.1
Party	6.57	.25	.47			
Race [black]	-11.37	1.62	-.12			
Region [South]	6.65	1.12	.10			
Constant	43.10	.91	—			
Mondale (number of cases = 2,010)						
Proximity model						
- Euclidean distance	4.77	.73	.12	.377	4.1	1.3
Party	-6.76	.23	-.54			
Race [black]	11.77	1.58	.14			
Region [South]	-.79[b]	1.13	-.01[b]			
Constant	83.35	1.41	—			
Directional model						
Scalar product	7.07	.63	.22	.402	19.1	3.8
Party	-6.05	.24	-.48			
Race [black]	7.57	1.58	.09			
Region [South]	-.94[b]	1.10	-.02[b]			
Constant	73.46	.87	—			

[a] The upper bound on variance explained is the variance explained when the issue variable is the only independent variable. The lower bound is the change in explained variance when the issue variable is included after the other independent variables.

[b] Not significant at the .01 level.

sion results for the full model and the two measures of variance explained. The upper bound is the variance explained when the issue variable is entered as the "first step" in the regression analysis, that is, when the issue variable is the only independent variable. The lower bound is the additional variance explained by the issue variable when it is included as the "last step" in the regression analysis, after the other independent variables of party, race, and region. Aside from the variance explained, the comparison of the standardized coefficients for the issue variables is of greatest interest. Since the distance and scalar product units are not comparable but each of the models is analyzed on the identical set of cases, the standardized coefficients are useful for comparing the effects of the two measures.

For both Reagan and Mondale the directional model is superior to the traditional proximity model. The directional model explains more variance, and the impact of a one-standard-unit change in the scalar product is greater than a one-standard-unit change in the distance measure. The standardized coefficient increases from .24 to .30 in the Reagan case and from .12 to .22 in the Mondale case. The increase in the magnitude of the standardized coefficient is 25% in the Reagan case and 83% in the Mondale case.

Table 2 shows the standardized coefficients and the variance explained for each of the 11 candidates. These values are extracted from the full regression analyses. The results consistently favor the directional model. In terms of the upper bound in percentage variance explained, every single case favors the directional model. Frequently quite large differences appear, both in proportional and absolute terms. Better than two-to-one ratios in variance explained occur for Mondale; Reagan in 1980; Kennedy; Ford; and Carter in 1976. Absolute differences in excess of 5% variance explained occur for Reagan in both 1984 and 1980; Mondale; Carter in 1976; and McGovern. As we would expect, the lower bounds show more muted results, but the general pattern remains distinctly in favor of the directional model. There is only one exception—Carter in 1980—and this by the slimmest margin of .1%. A comparison of the standardized coefficients for the issue variables underscores the point. Across the set of candidates, there is a median increase of 25% in the impact of issues using the directional model.

Mixed Model Analysis

An additional analysis will provide a more direct comparison of the empirical fit of the two models.[8] For this purpose we shall construct a general analytic model that encompasses both the pure proximity model and the pure directional model as well as any combination of the two. This mixed model is based on the calculating formula for Euclidean distance.[9] Using the notation we did earlier, the squared Euclidean distance between two points A and X can be written

$$|A|^2 + |X|^2 - 2|A|\ |X| \cos AX.$$

We see that the formula consists of three terms. The first two terms are the squared length of the candidate vector and the squared length of the voter vector. These two terms can be combined into a single term ($|A|^2 + |X|^2$), which we will call the length component. The third term is twice the scalar product. Thus, we can separate the vector length component of the distance formula from the scalar product component. We can then estimate empirically the importance of each in a single equation model.

We shall replicate the previous regression analyses of candidate evaluation, but now including as one independent variable the length component and as a separate variable twice the scalar product. The

Table 2. A Comparison of Proximity and Directional Models of Issues in Predicting Candidate Evaluation

Candidate Evaluation Models	Standardized Parameter Estimate	Adjusted R^2	% Variance Explained by Issues[a]		Number of Cases
			Upper Bound	Lower Bound	
Reagan 1984					
– Euclidean distance	.24	.451	19.3	4.8	2,080
Scalar product	.30	.474	26.2	7.1	2,080
Mondale 1984					
– Euclidean distance	.12	.377	4.1	1.3	2,010
Scalar product	.22	.402	19.1	3.8	2,010
Carter 1980					
– Euclidean distance	.12	.274	3.4	1.4	1,553
Scalar product	.12	.273	6.5	1.3	1,553
Reagan 1980					
– Euclidean distance	.13	.242	5.9	1.5	1,394
Scalar product	.23	.272	13.2	4.5	1,394
Anderson 1980					
– Euclidean distance	.18	.053	3.7	3.2	1,088
Scalar product	.20	.058	4.2	3.7	1,088
Kennedy 1980					
– Euclidean distance	.13	.191	4.7	1.5	1,332
Scalar product	.19	.206	10.3	3.0	1,332
Ford 1976					
– Euclidean distance	.07	.223	2.1	.4	2,069
Scalar product	.09	.226	4.4	.6	2,069
Carter 1976					
– Euclidean distance	.04	.253	.7	.1	2,020
Scalar product	.17	.278	9.9	2.5	2,020
Nixon 1972					
– Euclidean distance	.20	.278	9.4	3.7	2,499
Scalar product	.23	.292	10.4	5.0	2,499
McGovern 1972					
– Euclidean distance	.22	.311	9.8	4.4	2,405
Scalar product	.27	.331	17.5	6.4	2,405
Wallace 1972					
– Euclidean distance	.21	.215	10.8	3.6	2,231
Scalar product	.29	.254	15.6	7.6	2,231

Note: The parameter estimates and adjusted R-squared values come from a regression in which party identification, race, and region are also included.

[a] The upper bound on variance explained is the variance explained when the issue variable is the only independent variable. The lower bound is the change in explained variance when the issue variable is included after the other independent variables.

regression equation is

$$\text{candidate evaluation} = b_0 + b_1\text{length} + b_2\text{scalar} + \text{controls} + \text{error}$$

where length $= -|A|^2 + |X|^2)$, scalar $= 2|A|\ |X|\ \cos AX$, and controls $=$ party, race, and region.[10] Notice that if the unstandardized regression coefficient for the length term is equal to that of the scalar product ($b_1 = b_2$), the effect is the same as including the squared Euclidean distance between the voter and candidate points. Thus the model becomes a pure proximity model.[11] If the length term drops out, the effect is the same as including only the scalar product between the voter and candidate points, and the model becomes a pure directional model.

A test of the relative fit of the two models can be obtained by comparing the coefficients. For each candidate we shall compute the ratio of the unstandardized regression coefficient of the scalar product term to the coefficient of the length term, the *model ratio* $= b_2/b_1$. If the coefficients turn out to be approximately equal (a model ratio close to 1.0), the result will strictly favor the proximity model. If the vector length coefficient is small relative to that of the scalar product (a ratio well above 1.0), the result will favor the directional model. A result in which the coefficients are both significant but the scalar product coefficient is larger will suggest some type of mixed model.

Table 3 shows the results of this analysis. In every case the model ratio is well above 1.0, clearly favoring the directional model. The smallest ratios are for Nixon (2.5) and Carter in 1980 (2.6). The largest ratios are for McGovern (21.0) and Kennedy (20.5). Actually, the largest ratio is undefined because the length coefficient for Carter in 1976 has the wrong sign. In this case the length effect is in the direction *opposite* that implied by the proximity model. The median ratio is that for Reagan in 1984 of 8.6.

When we examine the regression coefficients, we see that in 9 of the 11 cases the coefficient associated with the vector lengths is of no substantial magnitude.[12] In each of these cases the standardized coefficient is considerably below .10. The exceptions are Anderson and Nixon. These two candidates show a significant length component, but in both cases the scalar product term is weighted more heavily. They are thus most consistent with a mixed model combining directional and proximity choice behavior. Why some element of proximity thinking enters into the evaluation of certain candidates is an intriguing question. But the analytic results leave no doubt that the dominant process of issue cognition follows more closely the directional model.

The best way to get a sense of the results from the mixed model analysis is to look at the curve for each of the candidates. Plots based on the regressions appear in Figure 4. On each graph the mean candidate location is marked by a * and the first letter of the candidate's name. Recall that a major difference between the theories is that traditional theory predicts that the support curve will peak at the candidate's position and decline from there, while directional theory predicts a monotonic relationship between respondent position and candidate evaluation.[13]

Looking at the figure, we see that the curves resemble the prediction of the directional model more than the traditional model. Six of the curves are virtual straight lines: Reagan in 1984; Mondale; Reagan in 1980; Kennedy; McGovern; and Wallace. Three curves are of modest slope and monotone: Carter in 1980; Ford; and Carter in 1976. These correspond well to the prediction for centrist candidates in directional theory. The Anderson and Nixon curves display a combination of directional and proximity effects but are more directional in character. They are only slightly nonmonotonic, and they peak at a position more extreme than the candidate's location.

The overall structure of these results strongly favors the directional over the proximity model. Of 11 comparisons, 9 fit the pure directional model and 2 a mixed model with the directional component

Table 3. A Comparison of Proximity and Directional Effects of Issues in a Mixed Model Analysis of Candidate Evaluation

Candidate Evaluation Models	Parameter Estimate	Standardized Parameter Estimate	Model Ratio[a]	Adjusted R^2	Number of Cases
Reagan 1984			8.6	.475	2,080
– squared lengths	.46[b]	.04[b]			
2 × scalar product	3.92	.29			
Mondale 1984			9.6	.402	2,010
– squared lengths	.38[b]	.03[b]			
2 × scalar product	3.63	.22			
Carter 1980			2.6	.275	1,553
– squared lengths	.68[b]	.06[b]			
2 × scalar product	1.80	.13			
Reagan 1980			10.9	.272	1,394
– squared lengths	.27[b]	.02[b]			
2 × scalar product	2.99	.23			
Anderson 1980			3.1	.085	1,088
– squared lengths	1.62	.17			
2 × scalar product	5.00	.22			
Kennedy 1980			20.5	.206	1,332
– squared lengths	.09[b]	.01[b]			
2 × scalar product	1.95	.19			
Ford 1976			4.5	.228	2,069
– squared lengths	.54[c]	.05[c]			
2 × scalar product	2.41	.09			
Carter 1976			undefined	.280	2,020
– squared lengths	–.63[c]	–.06[c]			
2 × scalar product	2.36	.17			
Nixon 1972			2.5	.309	2,499
– squared lengths	1.66	.15			
2 × scalar product	4.24	.29			
McGovern 1972			21.0	.331	2,405
– squared lengths	.12[b]	.01[b]			
2 × scalar product	2.52	.27			
Wallace 1972			6.5	.255	2,231
– squared lengths	.35[b]	.03[b]			
2 × scalar product	2.24	.31			

Note: The parameter estimates and adjusted R-squared values come from a regression in which party identification, race, and region are also included.

[a] This is the ratio between the unstandardized parameter estimates associated with two times the scalar product and the squared vector lengths.

[b] Not statistically significant at the .01 level.

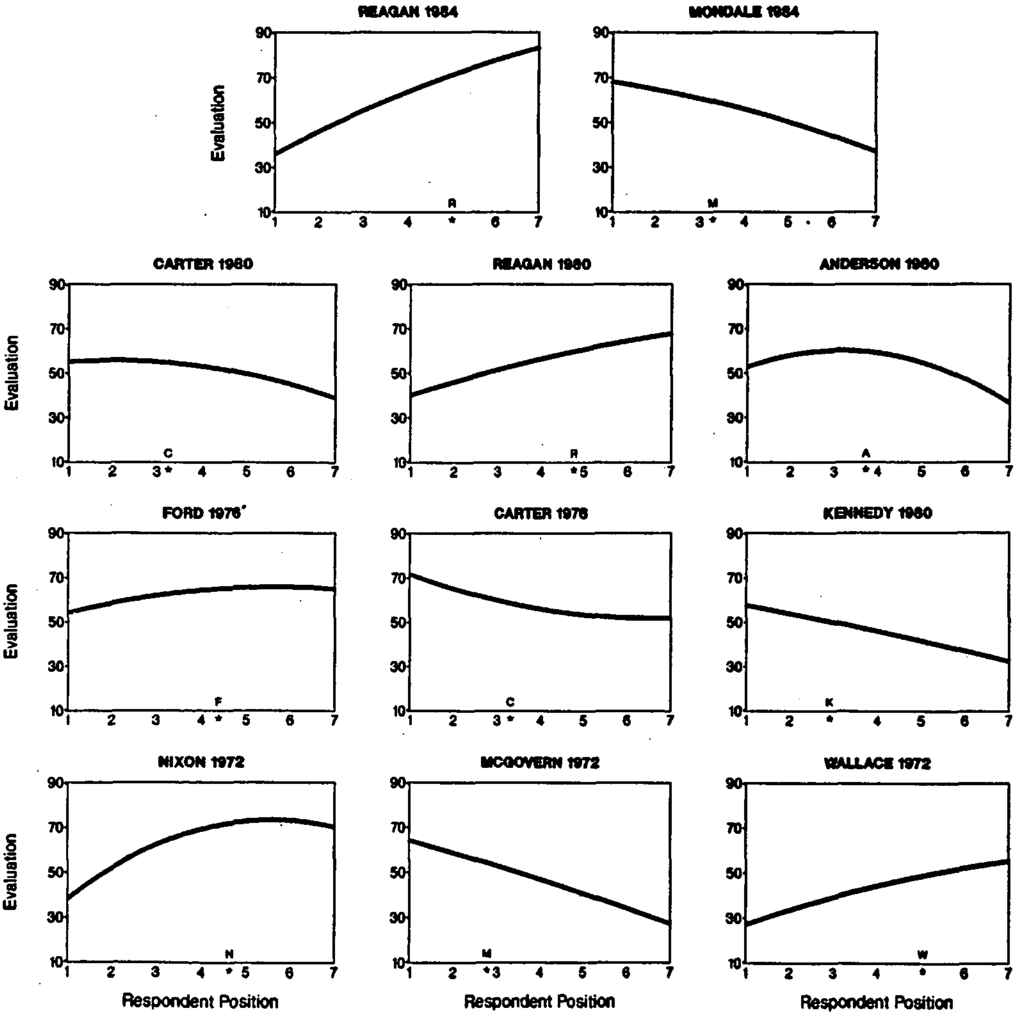
[c] Not statistically significant at the .001 level.

dominant. The pure proximity model does not fit a single case. When one considers that the issue questions used in the analyses were specifically designed to accommodate the traditional spatial paradigm, the results are all the more impressive.

A Directional Model of Electoral Competition

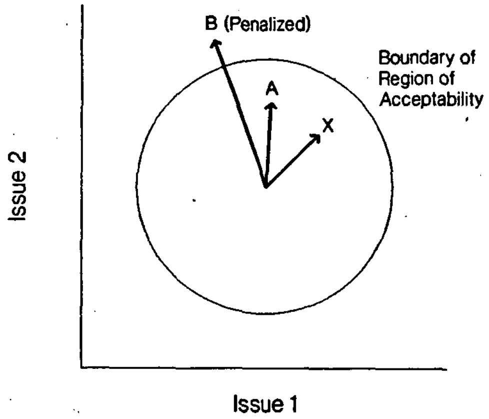
We have developed the directional model as a model of voter decision making. Now we wish to use the decision model as the basis for constructing a

Figure 4. The Estimated Relationship between Respondent Issue Position and Candidate Evaluation Using the Mixed Model



Note: For each candidate the full regression equation was used to construct the curve. The curve presented is for a political independent who is a white nonsoutherner.

Figure 5. Illustration of the Region of Acceptability



theory of electoral competition. To do this we must first consider the case of the very extreme candidate. The driving force behind the directional model is the idea that people react to issues in a diffuse way rather than with a concern for the specifics of policy and the exact position the candidate advocates. A candidate, however, must convince voters of his or her reasonableness. Voters are wary of candidates who seem radical and project harshness or stridency. The label "extremist" can attach to such candidates and severely hamper the enthusiasm of potential supporters. This idea is incorporated in directional theory by introducing the concept of the *region of acceptability.*

Figure 5 shows a directional space in which a circle is drawn around the center. The circle defines the region of acceptability. Any candidate who lies inside the circle is viewed as reasonable and avoids the extremist label. Any candidate who lies outside this region loses support by virtue of extremism. We assume in the ensuing discussion that any candidate whose intensity exceeds r, the radius of the region of acceptability, will perform worse in any two-person election than a candidate who adopts the same directional stand and is located at the boundary of the region of acceptability. We should be clear that this assumption does not mean that a candidate who is located beyond the region of acceptability will necessarily lose. The assumption simply states that a candidate will be penalized for crossing the boundary.

With the concept of the region of acceptability established, we are ready to develop the implications of the directional model for electoral competition. Just as the traditional spatial model suggests that candidates must act in a certain way to maximize votes, so too does the directional model. But the consequences for candidate behavior derived from directional theory are very different from those of the traditional spatial theory of elections.

Symmetry

A classic result from traditional spatial theory is the dominance of the mean position in two-candidate elections when the voter population is distributed in a symmetric fashion around the mean.[14] Under the assumption of symmetry and assuming at least one voter is located at the mean, a candidate located at the mean position will dominate a candidate located at any other position in the space. The intuition underlying this result is familiar from the unidimensional case. If a candidate wins the support of the person at the mean and everyone to one side of the mean, that candidate cannot lose; thus the mean position is dominant. Indeed, there are two principal implications derived from traditional spatial theory: (1) the dominance of the mean under certain assumptions and (2) the impossibility of determining a dominant position otherwise.

The directional model leads to quite different predictions. There are two theorems that are especially important, and these are proved in the Appendix. The first theorem is that if the electorate is symmetrically distributed about the origin (neutral point) of the space, any candidate within the region of acceptability is competitive with any other candidate within the region. In short, all strategies within the region of acceptability are equally desirable. This result follows from the fact that in any election in which two candidates have different issue stands, all those on one side of the neutral point will favor one of the candidates and all those on the other side of the neutral point will favor the other candidate. In the unidimensional case, for example, if one candidate is anywhere to the left of the neutral point and the other candidate is anywhere to the right, then all those on the left will favor the candidate on the left and all those on the right will favor the candidate on the right. If both candidates happen to be on the same side of the neutral point, the one who is more intense (but still within the region of acceptability) will receive the support of those on the same side, while the opponent will receive the support of those on the opposite side. Since symmetry implies an equal number on each side, the election will be a standoff.

A theoretical result like this is useful for the insight it provides. Conventional spatial theory has emphasized that the center is the most important position for a candidate when the electorate is evenly divided and issues are important to individual voting decisions. Under exactly these conditions, the directional model implies that a candidate may adopt a wide variety of issue positions that are just as desirable as the center. Indeed, issues are likely to play a very limited role in deciding who wins and loses in such an election. We should emphasize that a great deal of issue voting can still occur in this situation. It is just that issue voters who favor one side will cancel out those who favor the opposite side, and the eventual decision will be determined by factors such as the state of the times or candidate personality that have a more uniform impact across the electorate.[15]

Clear Directional Preference

The second theorem we wish to establish is that when the electorate has a clear directional preference, a dominant position exists and the dominant position is the most extreme position in the direction of that preference still lying within the region of acceptability. By a "clear directional preference" we mean that there is a position other than the center around which voters are symmetrically distributed with at least one voter at that position. Once again a formal proof appears in the Appendix.

The basic idea is readily illustrated in the unidimensional case. If more of the electorate is on one side of the neutral point than is on the other, the candidate who wins the support of those on the dominant side will win the election. We recall from the unidimensional example of directional theory that the candidate who is most appealing to those on his or her side of the issue is the candidate who provides the most stimulation by being intense. In the symbolic politics sense, the goal is to be the candidate who makes the most effective use of the symbol. However, there is a constraint on how aggressive a candidate should be, imposed by the concept of the region of acceptability. In one dimension the region of acceptability is a bounded interval centered at the neutral point. A candidate should not be so aggressive as to overstep the boundary of "reasonableness" or support will automatically be lost.

This second result is also a departure from traditional spatial theory. The theorem suggests that when the electorate has a clear policy preference, a candidate who responds to that preference will be rewarded. The strategic challenge for the candidate is to be as aggressive as possible in pushing the policy while still remaining within the region of acceptability. In contrast, traditional spatial theory would place the candidate at the mean voter location, generally a more moderate position than that suggested by directional theory.

In combination the two theorems suggest a distinctly different environment for electoral competition than traditional spatial theory. The thrust of directional theory is that candidates receive little reward for taking centrist positions. In the case of an electorate that is evenly divided on the critical issues of the day, even a relatively extreme strategy—so long as it remains within the region of acceptability—is competitive with a centrist strategy. In the case of a majority supporting a particular policy direction, a candidate who takes strong and aggressive stands in favor of that policy is likely to be advantaged against a centrist opponent.

Multiparty Systems

The general model also has implications for multiparty systems. While a full development of these implications is beyond our scope here, the predominance of multiparty systems among the world's democracies encourages a brief consideration. The directional theory implies that centrist parties will generally have a shaky and unstable support base in multiparty systems. Mathematically, this follows from the very short length of their "intensity" vectors, which implies that they will generate neither high affect nor high animosity. In systems where other competitors can stir strong affect, this leaves centrist parties electorally vulnerable. The theory suggests that the most successful parties will be those that are viewed as responsible but are intense enough to take advantage of the symbols appropriate to their principal constituencies.

Mixed Models

In the empirical analysis we introduced an analytic model that allows a mixture of directional and proximity behavior. From a theoretical standpoint, the mixed model represents a construct that embraces a variety of specific models depending upon the mix of the vector length and scalar product components. In order to understand better the theoretical implications of blending proximity and directional behavior, it will be useful to evaluate the entire class of models of this type. We do this in a rigorous fashion in the Appendix.

In general, the theoretical analysis shows that any particular mixed model becomes more proximitylike or directional in character as the blend between the components changes. From the standpoint of formal theory, the most important implication is that when there is a mixture of proximity and directional effects, the optimum candidate location is more extreme than the mean voter and bounded. This is very similar to the optimum strategy implied by pure directional theory, but in this case the boundedness is a characteristic of the mixed model. It does not require the assumption of the region of acceptability. Thus, for mixed models with a substantial directional component, the critical theoretical implications parallel those of the pure directional model and provide consistent predictions about the nature of the linkage between mass and elite.

Empirical Analyses of Mass-Elite Linkage

Virtually any theory of representative democracy must confront the issue of how the preferences of a constituency relate to the behavior of the officials the constituency elects. The traditional proximity model is well suited to the view of the representative as instructed delegate. In its purest form this theory implies that the representative will tend to behave as if he or she were the mean voter in the district. Directional theory suggests a different type of representation. When there is a clear directional preference in the district, the representative is predicted to be more extreme than the district in order to stimulate electoral intensity. In this sense, the representative may be said to "lead" the district. Directional theory also suggests considerable room for elite decision making when there is no clear directional preference. In those instances when opinion is evenly divided in the constituency, the representative is free to act like a trustee with no real constraint or guidance from the district.

A full empirical analyis of the representation question is beyond our scope here. Nevertheless, it is worthwhile to examine briefly the empirical evidence concerning the core theoretical implications of the two models. We will restrict our attention to the House and Senate in the United States. These provide a wide variety of constituencies and a sufficient number of representatives to allow systematic investigation. Relevant published work is available concerning the House of Representatives. Senate elections, however, are more visible than those for the House and are thus more likely to be decided on the basis of issues. When possible, therefore, we shall supplement the published work on the House with evidence from the Senate.

The Viability of Extreme Candidates

A key theme in directional theory is that candidates can compete successfully by taking extreme stands on issues. Indeed, in districts with a clear directional preference, candidates are advantaged by taking a position close to the boundary of the region of acceptability. In contrast, traditional proximity theory implies that candidates are most advantaged who mirror the preference of the mean voter. Which of these views appears to be more consistent with the empirical evidence?

House. The bulk of research on representation in the United States has dealt with the House of Representatives. Achen (1978) and Powell (1982) have attempted to specify in a technically precise way the relationship between representatives and the preferences of voters in their districts. Achen analyzed the 1958 Miller and Stokes data, while Powell analyzed 1978 campaign contributor data combined with National Election Study data and group ratings of representatives.[16]

In the Achen analysis, the losing candidate was closer on the average to the mean voter than was the winning candidate. Achen indicates that this result was produced by the fact that in many districts won by Republican congressmen, the winning candidate was substantially more conservative than the district represented. This is absolutely contrary to the predictions of the traditional proximity model and consistent with directional theory.

Powell, too, found that representatives tended to be significantly more extreme than the mean voter. In her analysis the representative was on the average approximately three times more extreme than the district mean. Powell speculates that this result is caused by the primary system. The argument is that competing in a primary pushes the candidate to a more extreme position than would occur if the candidate were competing directly in the general election. This explanation is certainly plausible, and it is useful to consider the logical consequences in terms of the two models. The traditional proximity model would predict that candidates who are incumbents and who do not face primary challenge should move to the district mean to ensure their victory in the general election. In contrast, directional theory would argue that no such movement should be necessary and that candidates can compete effectively over long periods by being ideologically more extreme than their constituency. While Powell did not draw these inferences, she did examine the relationship between seniority and proximity to the district mean. In general more senior representatives were found to be *farther* from their constituency rather than closer. This certainly accords better with directional theory than proximity theory.

Senate. There is no work on the Senate that is directly comparable to that of Achen and Powell on the House. However, Poole and Rosenthal (1984a) have presented evidence that the U.S. Senate is becoming polarized. Recent senatorial Americans for Democratic Action (ADA) and Americans for Constitutional Action (ACA) ratings are consistent with this observation. In 1984, for example, we find 60% of the senators rated in the extreme 0–24 and 76–100 ends of the rating scales, a result that appears out of step with the predictions of traditional proximity theory. Poole and Rosenthal explain the polarization in terms of the increasing power of interest groups. While interest groups may well be critical to defining ideological endpoints, a polarization such as Poole and Rosenthal describe makes sense in terms of mass-elite theory only if the capacity of a candidate to be electorally successful is not seriously diminished by the candidate's being extreme.

In this regard it is useful to consider two very salient recent examples. The 1984 senatorial election in which the most money was spent was the contest between Jesse Helms and James Hunt in North Carolina. Helms was acknowledged by virtually everyone (including himself) to be extremely conservative. His challenger was a popular centrist governor. Helms won the election. In 1986 the most expensive senatorial election pitted Alan Cranston, acknowledged to be quite liberal, against a moderate Republican, Ed Zschau. Again the more extreme candidate prevailed. In these elections it is likely that issues were important, and in both states the losing candidate was almost certainly ideologically closer to the mean voter than the winner. Hence, the outcomes violated traditional proximity theory. Yet both outcomes were consistent with directional theory: California is a moderate state and thus a state in which virtually any outcome is predicted to be possible. North Carolina is a moderately conservative state and thus a state where conservatives are predicted to be advantaged. While two cases hardly present a definitive argument, these were the most visible senatorial races of the last two elections. In both cases moderate challengers who appeared to be strong candidates lost to extreme opponents.[17]

The Permissiveness of Centrist Electorates

The other area in which the proximity model and the directional model differ in a fundamental way is the prediction regarding centrist electorates. The proximity model makes no distinction between centrist and more extreme districts. In all districts representatives are expected to follow the cues of the mean voter. In contrast, the directional model claims that electorates where preferences are fairly evenly balanced between the ideological poles are likely to exert *less* control over their representatives than electorates where one side of the ideological spectrum dominates.

House. Powell (1982) explicitly considered the question of whether centrist districts exert less control over the behavior of their representatives than more extreme districts.[18] In her analysis the directional prediction was entirely fulfilled. She found that representatives of centrist districts were on the average considerably farther from the district mean

Table 4. Analysis of the Relationship between State Ideology and the Ideological Behavior of Senators

Models	Parameter Estimate	Standard Error	Adjusted R^2	Number of Cases
Step 1 (Senator ideology)			.29	100
State ideology	6.02*	.93		
Constant	−10.60	5.62		
Step 2 (Level of error from step 1[a])			.11	100
Ideological extremity of state[b]	−2.47*	.68		
Constant	60.58*	4.10		

*Statistically significant at the .001 level.

[a] Absolute value of residual in predicting senator ideology.

[b] Absolute value of state ideology.

than those who represented more extreme constituencies.

Senate. Appropriate data are available to test the directional prediction in the Senate. To do this we require information about the ideology of each senator and the ideology of each state constituency. In the ensuing analysis we will use the difference between ADA and ACA ratings in 1984 as the basis for determining the ideology of senators, and the Rabinowitz, Gurian, and Macdonald (1984) ideological rating of state electorates to determine the position of the states.

The analysis requires two steps. First, we shall regress the ideological behavior of senators on the ideology of the state electorates in order to obtain a prediction of the behavior of each senator. The residuals or error values from this regression represent the extent to which a senator's behavior is incorrectly predicted by the state's ideology. Second, we will perform a regression in which the dependent variable is the magnitude of the residual and the independent variable is the ideological extremity of the state. Directional theory suggests that centrist states permit greater latitude in the ideological behavior of their delegates than more extreme states. Therefore, the more extreme the state, the lower the magnitude of the error should be. Conventional proximity theory suggests that no such relationship should exist. Hence, this regression serves as an empirical test between the theories.

The results of the analysis reported in Table 4 clearly support directional theory. The coefficient relating the ideological extremity of the state to the level of error in predicting senatorial behavior is −2.47, and it is significant beyond the .001 level. Since the ideological extremity of the states ranges from 0 to 15, this means that the ideological behavior of a senator representing a centrist state is on the average 37 points less predictable than the ideological behavior of a senator from an extreme state.

One possible criticism of this result, as well as the finding in the House, is that the prediction equation does not include a control for party. The inclusion of such a control is not suggested by either theory, since they do not predicate their predictions on the existence of a party system. Nevertheless, when a control for party is added, the effect is substantially diminished. Party accounts for a great deal of the unexplained behavior of candidates elected in centrist states.

How should this result be interpreted? According to traditional proximity theory, the party in its desire to win elections should encourage candidates to move to an optimum position. In a system like that in the United States, where candidates do not have to support an issue agenda set by the party leadership, there is little reason for the representative from a centrist district not to match the district mean. Traditional theory would certainly anticipate such matching. However, directional theory implies that there is no incentive to match the district mean and suggests that the parties can take quite distinct stands and still be electorally competitive. Unlike traditional proximity theory, directional theory is compatible with a system in which parties take distinct positions and representatives—even those from centrist districts—follow the party lead. Hence, the fact that party representatives take distinctly different ideological positions provides further support for directional theory.

The Empty Center

In examining the empirical evidence, there is one other finding in the literature that bears mention. Poole and Rosenthal (1984b) and Rabinowitz (1978) have published two-dimensional spatial maps in which voters and candidates are located in a common space. These analyses have utilized different statistical procedures, but both have relied on similar data and assumptions. The data have been individual affect ratings of candidates, and the assumptions have been those of the traditional proximity model. Thus, the scaling procedures have been designed to locate candidates near those voters who evaluate them most highly.[19]

These mappings of four presidential elections and more than 30 candidates have had one remarkable feature in common: not one candidate has been found to occupy the center of the space. The center position has always been dense with voters, yet devoid of candidates.[20] This result, inexplicable from the standpoint of proximity theory, is predicted by directional theory. According to directional theory, voters in the center do not have high affect for centrist candidates. Such voters have little interest in issues and such candidates provide little issue stimulation. Since centrist candidates do not receive high evaluations from those in the center, the scaling procedures must locate them in more peripheral parts of the space. Indeed, centrist candidates are often located away from any dense packet of voters because they fail to draw high affect from any group in the electorate.

Conclusion

From Stokes's (1963) early critique on, it has been clear to empirical researchers that the traditional spatial theory of elections is seriously flawed. Yet fully a quarter century later, that theory remains the dominant paradigm for understanding mass-elite linkage in politics. Our intent has been to present a formal theory of issue voting that has greater empirical verisimilitude. Based on the ideas of symbolic politics, the directional theory assumes that most people have a diffuse preference for a certain direction of policy-making and that people vary in the intensity with which they hold those preferences. When we compared the directional and traditional models empirically, we found that the directional model was better supported. We have also proposed a mixed model of issue impact that allows a blending of directional and proximity influences.

We conclude by considering a paradox of democratic political systems. Early empirical work stressed the failure of the mass public to control policy because of the lack of issue voting, and the general latitude in policymaking this gives elites (Berelson, Lazarsfeld, and McPhee 1954; Campbell et al. 1960). While some recent work has emphasized that issue voting is important, the conventional wisdom remains that most elections are decided on the basis of factors other than issues.[21] Page and Shapiro (1983), however, have shown that there is a clear tendency for policymaking to follow mass policy preferences. If there is little issue guidance through elections, as the conventional wisdom suggests, how does this relationship come about?

The directional model resolves this apparent paradox. Directional theory predicts no policy control when the electorate is evenly divided. Under that circumstance successful candidates can favor a wide range of policies. However, when there is a clear majority preferring an option, a politician favoring that preference will be advantaged. Thus, directional theory predicts considerable control by the mass under certain circumstances and absolute lack of control in other circumstances.

Rather ironically, mass control does not require high levels of issue voting. Issue voting occurs when politicians present different issue directions with sufficient intensity. An evenly divided and highly polarized electorate is the condition that should generate the most issue voting and the least real control of policy. Under this condition elections are virtual lotteries because the two sides cancel each other out, yet the policy consequences of the outcome are likely to be substantial. The election thus legitimates an "experiment" in a certain public policy. Under the condition of consensus, little issue voting will occur because politicians have every incentive to conform to the mass preference and few policy differences should emerge among candidates. The most meaningful policy guidance that results directly from election outcomes occurs when a majority exists for a policy, but that majority is either not obvious to politicians or is not courted by one of the parties for ideological reasons.[22] Under such conditions the election will tend to be won by the candidate espousing the majority direction.

Generally, parties can be expected to converge when there is consensus on the main issues of the day and diverge when polarization occurs. Directional theory thus implies a political system with the capacity both to experiment and to follow a direction encouraged by the mass public. Nevertheless, the situation from a normative perspective is far from sanguine. The system can allow for almost random turnover of policy and the instructions that elites receive from the mass are nonspecific. This is pretty much where the careful empirical analyses of the early voting studies left us. In this regard we have come almost full circle, but we believe we have arrived with a better understanding of the role of issues in selecting leaders.

Appendix

This discussion assumes a basic knowledge of analytic geometry and calculus. A source for the mathematics used in this section is Thomas 1960.

Definitions and Notation

Mixed model. In a mixed model, $U_x(A) = k_{sp}[2X * A] - k_{len}[|X|^2 + |A|^2]$, where

$U_x(A)$ = the utility of voter X for candidate A. When there is no danger of confusion, this will be written simply as U(A).

$X * A$ = $\Sigma(x_i a_i)$. Notice that uppercase letters are used to represent vectors and lower case letters their associated coordinates, which are real numbers. The symbol * is used to indicate the scalar product between two vectors. The letters A and B are used to represent candidates and the letter X to represent an arbitrary voter.

k_{sp} = a constant equal to the weight of the scalar product component in the mixed model.

k_{len} = a constant equal to the weight of the length component in the mixed model.

$|X|$ = $(\Sigma x_i^2)^{1/2}$, the length of the X vector.

Clear directional preference. An electorate is said to have a clear directional preference if the electorate is symmetrically distributed about a point M, where M is not the origin, and at least one voter is located at M.

Region of acceptability. The region of acceptability is the set of points P, such that $\Sigma p_i^2 \leq r^2$, where $r > 0$ and r is called the radius of the region of acceptability.

Observations and Lemma

Observation 1. Both scalar product and squared distance models are special cases of a mixed model. A pure scalar product model holds when $k_{len} = 0$, and a pure squared distance model holds when $k_{sp} = k_{len}$. Distance is a monotonic function of squared distance.

Observation 2. Utility calculated for a mixed model is rotationally independent.

Discussion. Other than constants, the mixed model depends only on the length of vectors and their angular separations, which are invariant under rotation.

Observation 3. In a mixed model, the preference of a voter X for candidate A over B can be written in either the form $X * D > k_{elec}$ or $X * D/|D| > k_{int}$, where $D = A - B$, and k_{elec} and k_{int} are constants once the positions of A and B are established.

Discussion. Consider the utility of voter X first for candidate A and then for candidate B according to the mixed model and then calculate their difference. This difference which determines preference has the desired form. We show the calculations below:

$$\begin{aligned} U(A) &= k_{sp}[2X * A] \\ &\quad - k_{len}[|X|^2 + |A|^2] \\ U(B) &= k_{sp}[2X * B] \\ &\quad - k_{len}[|X|^2 + |B|^2] \\ U(A) - U(B) &= k_{sp}[2X * D] \\ &\quad - k_{len}[|A|^2 - |B|^2] \end{aligned}$$

$U(A) > U(B)$ implies that A is preferred to B. Hence, when A is preferred, $U(A) - U(B) > 0$; $k_{sp}[2X * D] - k_{len}[|A|^2 - |B|^2] > 0$; $X * D > k_{elec}$, where $k_{elec} = k_{len}[|A|^2 - |B|^2]/2k_{sp}$; or $X * D/|D| > k_{int}$, where $k_{int} = k_{elec}/|D|$.

Observation 4. A hyperplane divides the supporters of A from those of B. In a pure scalar product space that hyperplane runs through the origin.

Discussion. This follows directly from observation 3 and the definition of a hyperplane and, in the case of the scalar product model, the fact that $k_{elec} = 0$ because $k_{len} = 0$.

LEMMA. In a mixed model, if the electorate has a clear directional preference, any strategy that is optimal for M is optimal overall.

Proof. Let A be preferred to B by voter X; hence $X * D > k_{elec}$ (observation 3). Assume M is on the plane; thus $M * D = k_{elec}$. Subtracting $M * D$ from the left and its equivalent k_{elec} from the right side of the initial inequality, $X * D - M * D > 0$ and $(X - M) * D > 0$. If $(X - M) * D > 0$, then $-(X - M) * D < 0$. Now adding $M * D$ to the left and k_{elec} to the right side of the inequality, we have $[-(X - M) + M] * D < k_{elec}$, or $X' * D < k_{elec}$, where $X' = -(X - M) + M$ is the symmetric opposite of X with respect to the point M. Thus, starting with the assumptions that M is on the hyperplane and that X prefers

A, we have established that X′, the symmetric opposite of X, will prefer B. Hence, any hyperplane that includes M will have the same number of voters on each side.

Now consider a parallel shift of any of those hyperplanes. The side that includes M must now include more than one-half the voters, because M is included. Since every hyperplane is parallel to a hyperplane that runs through M, the side that includes M must include more than one-half the voters.

Finally, consider the fact that if $U_M(A) > U_M(B)$, M lies on the A side of the hyperplane. Since A will win the election with M's support and will lose the election if M supports B, any strategy that is optimal for winning the support of M is optimal overall.

Directional Theorems

Theorems 1 and 2 both assume that the utility function defining voter affect is scalar product in the region of acceptability and that any candidate whose vector length exceeds *r*, the radius of the region of acceptability, will perform worse in any two-person election than a candidate who adopts the same directional stand and is located on the boundary of the region of acceptability.

THEOREM 1. When the electorate is symmetrically distributed about the origin (neutral point) of the space, any candidate within the region of acceptability is competitive with any other candidate within the region.

Proof. Let A be preferred to B by voter X, then $X * D > 0$ (observations 3 and 4). Now by virtue of the symmetry assumption there exists a voter $X' = -X$. For X′ the preference must be B since $-X * D < 0$. Hence for every supporter of A there is a matching supporter for B and the election will be tied.

THEOREM 2. When the electorate has a clear directional preference, a dominant position exists: and the dominant position is the most extreme position in the direction of that preference still lying in the region of acceptability.

Proof. Restrict attention to the region of acceptability, since by assumption any strategy outside the region will perform worse than a strategy with the same directional preference but lying on the boundary of the region. Without loss of generality, rotate the space so the directional preference is oriented along the first dimension (observation 2). We will represent this preference as $M = (m_1, 0, 0, \ldots, 0)$. Now $M * A = \Sigma m_i a_i = m_1 a_1$ for any arbitrary candidate A, since $m_i = 0$ when $i > 1$ and $m_1 > 0$, since a clear directional preference exists. The highest possible scalar product within the region of acceptability is obtained when A adopts the position $(r, 0, 0, \ldots, 0)$, since *r* is the largest possible value for the first coordinate and that coordinate must be less than *r* unless all the remaining coordinates are 0. Since $(r, 0, 0, \ldots, 0)$ is the optimum strategy for winning the support of M, it is the optimum strategy overall (see the lemma).

Mixed Model Theorems

Theorems 3 and 4 assume that the utility function is that of the mixed model.

THEOREM 3. When the electorate has a clear directional preference, the optimum strategy for a candidate is to occupy the position that is in exactly the same direction as the mean voter, M, but one that is more extreme than the position of the mean voter by a factor equal to the scalar-product-to-length ratio.

Proof. The position of the mean voter, M, is $(m_1, m_2, \ldots, m_n)$, where *n* is the dimensionality of the space. The utility of an arbitrary candidate A for M is $k_{sp}(2\Sigma m_i a_i) - k_{len}(\Sigma m_i^2 + \Sigma a_i^2)$. Differentiating with respect to a_i and setting the result to zero, we have $2k_{sp} m_i - 2k_{len} a_i = 0$, or $a_i = (k_{sp}/k_{len}) m_i$. Hence $A = (k_{sp}/k_{len})M$ is an optimal strategy for M and thus an optimal strategy overall (see the lemma).

THEOREM 4. When the electorate is symmetrically distributed around the center, the center position is dominant. However, the advantage that accrues to the center candidate declines monotonically as the ratio of the scalar product to the length component increases.

Proof. If the dividing hyperplane passes through the origin, the symmetry assumption assures that the election is tied. The dividing hyperplane is perpendicular to the vector $D = A - B$ by virtue of its equation, $X * D = k_{\text{elec}}$ (see observation 3). As the hyperplane is shifted away from the center toward the more extreme candidate, the number of voters on the centrist side can only increase. Similarly, movement of the hyperplane towards the center candidate can only decrease that candidate's support. The value k_{int} defined in observation 3 represents the distance the hyperplane is shifted. (To see this, simply consider the rotation where $D = (d_1,0,0, \ldots, 0)$, and hence $D/|D| = (1,0,0, \ldots, 0)$. Under this rotation the equation of the hyperplane is $x_1 = k_{\text{int}}$.) The value of k_{int} is inversely proportional to the ratio of $k_{\text{sp}}/k_{\text{len}}$. Hence the support advantage to the center candidate must decline monotonically as the ratio increases.

Notes

Howard Rosenthal provided a critical yet extremely valuable commentary on an early draft of this paper at the Carnegie-Mellon Conference on Political Economy, 1987. He deserves much of the intellectual credit for the concept of the mixed model that we develop in the text and Appendix. William Keech and Barry Nabiloff also provided very helpful comments on that early draft. We are indebted to the Fulbright Program and the Department of Sociology of the University of Trondheim, Norway, for providing us support and encouragement during the 1987–88 academic year. Finally, we wish to thank the Center for Political Studies at the University of Michigan for making available the data we have analyzed in this paper.

1. For an excellent summary of the relevant theoretical literature see Enelow and Hinich 1984.

2. Niemi and Bartels (1985) used the reported importance of each issue in their salience-weighted model. By selecting only the issue each respondent feels is most important, a measurable impact of salience is likely to be found (see Rabinowitz, Prothro, and Jacoby 1982). The main point for our discussion, however, is that much of the salience information is captured in the response to the issue question itself, and that is reflected in the Niemi and Bartels result.

3. The equivalence between these two formulas follows directly from the law of cosines. For a proof see Thomas 1960 (p. 610).

4. The concept of intensity could be broken down into two components: (1) the certainty of directional preference and (2) the salience of the issue (personal importance). Under this decomposition we arrive at a direction-certainty-salience model that is analytically equivalent to the model we present here. In this model the length of the voter vector is interpreted as the product of the certainty of voter preference and the salience of the issue to the voter, while the length of the candidate vector is the product of the certainty that the candidate will pursue a given policy direction and the stress placed on the issue. These ideas will be developed more fully in future work. We are grateful to Barry Nabiloff for encouraging us to think of the probabilistic structure of the model.

5. The issues are as follows: 1972, government jobs, tax rate, Vietnam, inflation, marijuana, busing, medical insurance, pollution, rights of accused, minority aid, liberal-conservative, urban unrest, and student unrest; 1976, government jobs, rights of accused, busing, minority aid, medical insurance, liberal-conservative, urban unrest, marijuana, tax rate, and women; 1980, liberal-conservative, defense spending, government services, inflation and unemployment, minority aid, Russia, women, government jobs; 1984, government services, minority aid, Central America, defense spending, women, Russia, government jobs, and liberal-conservative.

6. The fact that the directional model is vector-oriented rather than point-oriented requires that the center of the space (the origin of the vector) be delineated. In the analysis we shall insist that the middle value (location 4) on the seven-point scale serve as the center in order to use no more information in calculating potential affect in the directional model than in the traditional spatial model.

The exact calculations we shall perform are the following:

average distance of voter i to candidate k =

$$[\Sigma (x_{ij} - a_{kj})^2 / n_i]^{1/2}$$

average scalar product of voter i with candidate k

$$= \Sigma\,[(x_{ij} - 4)(a_{kj} - 4)]/n_i,$$

where x_{ij} is the position of voter i on issue j, a_{kj} is the mean position of candidate k on issue j, and n_i is the number of issues on which voter i locates both him- or herself and candidate k.

7. While many empirical analyses of voting have relied directly on the voter's perception of candidate location, this is not consistent with the assumptions of the traditional proximity model (see, e.g., Davis, Hinich, and Ordeshook 1970, 431). The major contribution of spatial theory is to provide a clear link between the issue strategies of candidates and the preferences of the masses. To rely directly on perceptions and provide no constraint on the "reality" of that perception is inimical to the theory and vitiates much of its value.

The role of perception can, of course, be a focus of substantive interest. Enelow and Hinich (1984) include a model of voter perception in their revised spatial theory of voting. Their elaboration entails a complex transformation of the data matrix that leads to estimation of both individual and candidate positions in a reduced-dimensionality space. A model of perception could be developed and blended with the directional theory as well. Such a task, however, is beyond our scope here. In addition, the seven-point issue scales appear to be so heavily cued to a proximity response (Brody and Page 1972; Markus and Converse 1979; Page and Brody 1972) that they are unlikely to provide a good basis for empirical investigation of the perceptual process.

8. We are indebted to Howard Rosenthal for suggesting this method to us.

9. The squared Euclidean distance between two points is $\Sigma\,(a_i - x_i)^2$, where i indexes the dimensions of the space. To see the equivalence between this and the formula we present, consider the following expansion:

$$\begin{aligned}\Sigma\,(a_i - x_i)^2 &= \Sigma\,a_i^2 + \Sigma\,x_i^2 - 2\,\Sigma\,a_i x_i \\ &= |A|^2 + |X|^2 - 2|A|\ |X| \cos AX.\end{aligned}$$

10. The signs of the length and scalar product variables are reversed in order to make the expected regression coefficients positive.

11. We have performed a comparable analysis on the square root of the sum of the two measures. Using the square root, when the coefficients are equal, the result is exactly equivalent to distance instead of squared distance. The analysis involving the square root, however, is far more complicated because it entails nonlinearity in the parameters. We estimated this model using a maximum likelihood strategy, and the results obtained were quite similar to those we report based on the squared distance.

12. With samples in excess of one thousand in every case, statistical significance is not the best way to assess the empirical importance of a coefficient. If we apply a fairly stringent .001 criterion, 9 of the 11 length coefficients are statistically insignificant. If we use a less stringent .01 significance level, 7 of the coefficients are statistically insignificant. The 2 coefficients that pass the less rigorous test are the Ford and Carter coefficients in 1976, the Carter coefficient having the wrong sign.

13. Since squared distance is used in the mixed model analysis, the shape of the proximity curve is parabolic, peaking at the candidate's position.

14. In a strict sense the critical center position in spatial theory is the median rather than the mean. In symmetric electorates they are, of course, the same. For convenience we will generally use the term *mean* when referring to the center of the distribution.

15. See the discussion of skewness in Butler and Stokes (1969, 187–92) for an interesting parallel argument.

16. In using roll call behavior to indicate candidate position in a directional space, we are assuming that policy behavior is directly related to issue intensity. It is likely that proximity theory applies to elites and directional theory applies to the masses and that they are linked through the process of election campaigns.

17. The results of a systematic analysis of the 1982 senatorial elections by Wright and Berkman (1986; see especially the comments of Bernstein in Bernstein, Wright, and Berkman 1988) are generally consistent with the directional model, although the authors do not interpret them in that way.

18. Powell's interest in the question was motivated by the representation theory of Fiorina (1974). Directional theory predictions, which are based on an individual-level choice model, are compatible with Fiorina's theory of elite behavior.

19. Rabinowitz' (1978) scaling procedure is nonmetric and respondents are located exclusively in terms of their rank-order preference for candidates. Hence, the results can not be an artifact caused by the propensity of individuals to interpret the numerical scales in different ways. The similarity between Poole and Rosenthal's (1984b) and Rabinowitz' (1978) findings supports the general reliability of each.

20. Some candidates (e.g., George Romney and Henry Jackson) have attempted centrist strategies and have been located close to the center on ideological rating scales.

21. For good discussions of the role of issues in elections see Abramson, Aldrich, and Rohde 1986 and Asher 1984.

22. Chappell and Keech (1986) and Wittman (1983) are among those who present models in which elite policy motivations encourage resistance to optimal spatial position.

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George Rabinowitz is Professor and Stuart Elaine Macdonald is Assistant Professor of Political Science, University of North Carolina, Chapel Hill, NC 27514.

FOREIGN AFFAIRS AND ISSUE VOTING: DO PRESIDENTIAL CANDIDATES "WALTZ BEFORE A BLIND AUDIENCE?"

JOHN H. ALDRICH
Duke University

JOHN L. SULLIVAN
EUGENE BORGIDA
University of Minnesota

While candidates regularly spend much time and effort campaigning on foreign and defense policies, the thrust of prevailing scholarly opinion is that voters possess little information and weak attitudes on these issues, which therefore have negligible impact on their voting behavior. We resolve this anomaly by arguing that public attitudes on foreign and defense policies are available and cognitively accessible, that the public has perceived clear differences between the candidates on these issues in recent elections, and that these issues have affected the public's vote choices. Data indicate that these conclusions are appropriate for foreign affairs issues and domestic issues.

> Foreign policy attitudes among most Americans lack intellectual structure and factual content. Such superficial psychic states are bound to be unstable since they are not anchored in a set of explicit value and means calculations or traditional compulsions.
>
> —Gabriel A. Almond

> The public's attention span to foreign affairs is strictly limited; elections are not decided on foreign policy issues.
>
> —Deborah Welch Larson

Almond's (1950) research indicated that most U.S. citizens knew very little about the remote issues of foreign policy and instead focused their attention on domestic policies. They did so because domestic issues often have more direct and immediate consequences on people's lives and their material well-being. This view continues to be widely held today, and as a consequence studies of public opinion and voting tend to focus on domestic issues. To be sure, there is a dissenting view, but the study of issues remains primarily the study of domestic issues.

This view that voting ends at the water's edge presents something of an anomaly. Many presidential candidates seem to place considerable emphasis on foreign affairs. If these issues are unimportant to the public, why do candidates waste their scarce time and resources addressing them? Are they basing their campaign behavior on a false assumption? Are they waltzing before a blind audience?

Foreign Policy Attitudes: The Prevailing Scholarly View

Most research on the 1984 election treats foreign policy lightly or ignores it altogether (Abramson, Aldrich, and

Rohde 1987; Boyd 1985; Burnham 1985; Ladd 1985; Shanks and Miller 1985; Weisberg 1985). Light and Lake (1985, 94), for example, note that "Public opinion polls . . . show that people do not follow foreign affairs closely and often do not know enough about the specifics of a particular issue to form opinions." They conclude that "the [1984] election was clearly a referendum on the state of the economy."

Studies like those of the 1984 election are not unique. As early as 1949, scholars concluded that about 30% of the public was unaware of foreign issues, another 45% was aware but unable to frame an intelligent argument, and the remaining 25% had some knowledge of foreign problems (Almond 1950). These groups have been labeled the "mass public," the "attentive public," and "opinion leaders" (Hughes 1978). Rosenau (1961) estimated the size of these three groups at about 75%, 15–20%, and 5–10%, respectively, an even more pessimistic assessment than the one offered more than a decade earlier.

Some scholars have suggested that the public's ill-formed opinions on foreign affairs have no impact on their voting behavior. Kagay and Caldeira (1975), for example, extended Stokes's (1966) analysis of the open-ended likes-dislikes questions and found that foreign policy had a modest effect on the electoral outcomes in 1952 and 1956 but had virtually no impact between 1960 and 1976. Only in 1972, with the Vietnam War so important, did foreign affairs have a significant effect.

Hess and Nelson (1985, 143–44) concluded that foreign policy had a significant impact on electoral behavior only in 1952, 1972, and 1980. In these three elections the Korean and Vietnam Wars and the Iranian hostage crisis exemplified "intermestic" issues: foreign issues that had a strong domestic component that affected daily life. They also argued that foreign policy played its typically minor role in the 1984 election. But it is Hess and Nelson who pose the anomaly reflected in our title. In virtually all recent elections, they argue, foreign policy has been the candidates' dominant concern. While this may have been less true in 1984, they suggest that it was still important during the nomination campaign and in the final Mondale-Reagan debate on foreign affairs. They argue, however, that in no recent campaign, including 1984, did foreign policy issues have a decisive impact on voting behavior.

One way to resolve the anomaly is to conclude that candidates simply misunderstand the role of foreign policy issues in presidential elections.[1] But a second resolution is to conclude that the prevailing scholarly wisdom underestimates the importance of foreign policy attitudes. Reagan, for example, made significant gains in his Florida primary campaign against Ford in 1976 after his decision to shift his emphasis from domestic issues to foreign policy.[2] Richard Wirthlin's polls showed that after the shift in focus, Reagan cut Ford's lead in Florida by 14 percentage points (Witcover 1977).

There is some scholarly evidence suggesting that foreign policy issues do play an important role in electoral behavior. There have been a few scholars who have argued that foreign policy attitudes play an enduring and important role in electoral behavior. Kessel (1988) analyzed the likes-dislikes data from the 1952–84 period and found that his "international involvement" measure was strongly related to the vote in all but the 1968 and 1976 elections. Its impact exceeded the effect of most issues in virtually all elections and even exceeded "economic management" in several elections (for similar data through 1980 see Miller and Wattenberg 1981). Hurwitz and Peffley (1987) show that foreign policy, if measured as perceptions of general retrospective "postures" taken by the administration, exerted a substantial impact on Reagan's approval ratings. Other scholars have reported isolated findings suggesting that there are exceptions to the conventional wisdom (Miller and Shanks 1982; Nie, Verba, and Petrocik 1976; Page and Shapiro 1983; Pomper 1972).

The *prevailing* consensus, however, is that the public possesses little information and only few, ill-formed attitudes about foreign affairs and is concerned deeply about these issues only when their daily lives are directly affected. As a result, such concerns are not terribly consequential in the voting booth. It seems clear, however, that a reexamination of the prevailing consensus is warranted.

Issue Salience, Attitude Accessibility, and Voting Behavior

The prevailing scholarly consensus—that foreign policy attitudes are either nonattitudes or weakly held attitudes—has an intuitive plausibility given the remote and complex nature of foreign policy issues. But there has been no theoretically plausible account of attitude formation and salience that would explain why attitudes on domestic issues—as opposed to foreign policy issues—should be so accessible and so likely to affect voting behavior. We will review recent work in social psychology on attitudes and their relationship to behavior that can provide such an account.

During the late 1960s and early 1970s the conceptual and predictive utility of attitudes was seriously questioned by many social psychologists (e.g., Abelson 1972). However, a second "generation" of attitude research beginning in the mid-1970s reexamined the relationship between attitudes and behavior by adopting a moderator variable approach (Fazio and Zanna 1981). Instead of asking the question "Do attitudes predict behavior?" scholars began to investigate more systematically the various conditions, attitudinal qualities, individual differences, and behavioral criteria that moderate attitude-behavior relations. More optimistic assessments of the conceptual and predictive utility of the attitude construct have emerged from these studies.

More recent research examines the *process* by which attitudes guide decision making and behavior. The emergence of these process models may bring some theoretical coherence to the burgeoning set of variables that seem to moderate the relationship between attitudes and behavior. The fundamental assumption underlying the various process models (e.g., Chaiken 1987; Fazio 1986; Snyder 1982) is that available or existing attitudes must first be made accessible from memory if they are to exert any influence on perceptions, judgments, or behavior. *Availability* refers to whether a construct or category is stored in memory (Higgins and King 1981), while *accessibility* refers to the readiness with which a stored construct like an attitude is retrieved from memory or is used in stimulus encoding.

Thus Fazio's (1986) model of attitude accessibility, the most developed of the attitude-behavior process models in current research on social cognition, posits that available and accessible attitudes are more likely to guide the processing of relevant information and behavior than are less accessible attitudes. Thus attitudes may be thought of as occupying a position on an accessibility continuum. At the lower end of this continuum are nonattitudes, where no evaluation of the attitude object is represented in memory. At the upper end of the continuum are attitudes that are well learned and chronically accessible. In the middle range of the continuum are attitudes that are available but only moderately accessible. Empirical tests of this model have shown that the accessibility of attitudes determines the predictive power of an attitude. Accessibility, in turn, can vary depending on both chronic and temporary factors. The *chronic accessibility* of constructs like attitudes refers to whether the construct is frequently and consistently activated from memory upon mere exposure to the attitude object. This is distinguished from *temporary accessibility*, in which the construct is temporarily "primed," or rendered more accessible, by the current environmental context.

Research in social cognition suggests that both chronic and temporary sources of construct accessibility influence the interpretation of social information (e.g., Bargh et al. 1986). Attitudes that become temporarily accessible, for example, may be particularly important in an electoral context. Candidates or media emphasis on foreign policy issues may temporarily increase the accessibility of such attitudes for members of the public whose attitudes are not chronically accessible. Continuous campaigning on these issues could prolong the otherwise short-term accessibility of situationally primed attitudes. Thus to the extent that foreign policy attitudes are available, attitude accessibility models predict that such attitudes are more likely to influence voter decision making (Fazio and Williams 1986). In addition, to the extent that foreign policy attitudes are available but are not chronically accessible, process models predict that the impact of these attitudes on voter behavior will be enhanced by environmental factors that increase temporary accessibility.

The method of attitude formation is one important factor that affects the evaluative strength of attitudes (Fazio 1986; Fazio and Zanna 1981). Attitudes based on indirect experiences with an attitude object are as available in memory as attitudes based on direct behavioral experiences. But attitudes based on direct experience are more *accessible* from memory; are held with greater confidence and certainty; have more complex, multidimensional attitude structures; have stronger links to behavior; and are more resistant to persuasion than attitudes based on relatively indirect experience (Fazio et al. 1982; Fazio and Zanna 1981; Schlegel and DiTecco 1982; Wu and Shaffer 1987).

One explanation, then, for the conventional wisdom that domestic attitudes are important while foreign policy attitudes are not is that the former are more accessible because they are more likely to be based on direct experience. The public is likely to have direct experience with problems such as inflation and unemployment, but they are unlikely to have direct experience with arms negotiations or U.S. policies toward Central America. The heavily technical nature of military security and international economic issues is also said to play a role.

But this view may be too facile. Many U.S. citizens have experienced, directly and indirectly, the impact of three major hot wars and one prolonged cold war. A significant proportion of us have matured under the anxiety of the bomb and conditions threatening nuclear war. These experiences, whether direct or vicarious, are certainly as potent as ordinary fluctuations in the macroeconomy. Just as a dramatic economic event like the Great Depression shapes the attitudes and behaviors of a generation for a lifetime, so too have war and the presence of nuclear weapons affected all current generations for a lifetime. Since attitudes toward foreign policies are heavily imbued with rather concrete nuclear war images (Fiske, Pratto, and Pavelchak 1983) and personal fears (Kramer, Kalick, and Milburn 1983), it is likely that they are as accessible as most attitudes about domestic issues.

Indeed, as Fazio (Fazio 1987; Fazio, Herr, and Olney 1984) has shown, people can have direct behavioral experience with social issues—foreign or domestic—by virtue of their engaging in some behavior relevant to the issue and having the opportunity to infer their attitudes from their behavior. For example, if an individual has discussed a given foreign or domestic issue, taken a public stand, signed a petition, or done anything of this sort, the person, Fazio would argue, acquires through this process of self-perception direct behavioral experience with the attitude issue. Again, attitudes based on such behavioral experiences have been found to be highly accessible and more predictive of later behavior than are attitudes based on indirect experiences with the attitude object.

This argument suggests that the candidates' assumption that foreign policy issues are important in presidential campaigns is valid. There is little *theoretical* reason to expect large differences in the availability and campaign accessibility of foreign and domestic attitudes. In fact, the candidates' emphasis on foreign policy may *ensure* equal accessibility. First, we will test this expectation by assuming that most people hold basic, general views in foreign affairs, but that they are—and should be expected to be—largely unconcerned about the detailed arguments that characterize elite debate on most of these issues.[3] Such debates matter to the public only if they illustrate the candidates' more general stances or personal qualities.

Second, we will assess whether foreign policy attitudes are available and whether their accessibility varies from election to election. For instance, peace is a central and ordinarily consensual goal. When events, candidates, or media coverage indicate a threat to peace, the salience of this issue increases, which in turn enhances the accessibility of attitudes about peace. Third, above and beyond the availability and accessibility of foreign policy attitudes, we will examine their relevance for models of voting behavior. We will do so by exploring whether the public perceives significant differences between the candidates on foreign policy issues and whether these perceived differences have a direct impact on voting. This allows us to compare the prevalence of "issue voting" on foreign and defense issues with domestic issues. Throughout the analysis, the standard of comparison for the nature and impact of foreign policy attitudes will be the nature and impact of domestic policy attitudes.

The Availability of Foreign Policy Opinions

In order for foreign policy attitudes to play an important role in voting in presidential elections, they must be available to much of the public, that is, they must be represented in memory. We will analyze data from the 1980 and 1984 NES and a 1984 Gallup survey[4] to establish whether foreign policy attitudes are as available as attitudes on domestic issues.[5]

Determining the availability and the "quality" of the public's attitudes is of course problematic. Since we are interested ultimately in the relationship between attitudes and voting behavior, we can assess the set of conditions that Campbell and his colleagues argued were necessary preconditions for issue voting. These conditions are that the issue must (1) be cognized, (2) arouse some affect, and (3) be accompanied by the perception that one party or candidate best represents the respondent's position (Campbell et al. 1960, 170). Scholars assessing these conditions have debated whether voters are becoming more issue-oriented (Margolis 1977; Pomper 1972, 1975). The conditions themselves represent controversial tests of the voters' competency, and scholars have debated whether these are jointly necessary conditions for issue voting (Abramson, Aldrich, and Rohde 1983, 1987; Aldrich and Trump 1986; Fiorina 1981). These conditions do, however, provide a minimum indication of the quality of the public's attitudes. They are especially useful for the task of comparing the public's foreign and domestic policy attitudes, because the point is not so much the absolute quality of foreign policy attitudes as

Table 1. Average Percentages Meeting Various Issue Voting Criteria, 1980–84

Issue Scale	Placed Self on Issue Scale	Placed Self & Both Candidates	Placed Self & Saw Difference between Candidates	Placed Self & Saw Democrat As More Liberal than Republican
1980 (NES)				
Foreign (2 scales)	84.8	70.2	61.2	52.6
Domestic (5 scales)	80.9	64.1	53.7	44.4
Average difference[a]	3.9	6.1	7.5	8.2
1984 (NES)				
Foreign (3 scales)	83.7	72.0	62.8	54.5
Domestic (4 scales)	86.4	75.7	63.3	53.2
Average difference[a]	-2.7	-3.7	-0.5	1.3
1984 (Gallup)				
Foreign (2 scales)	92.6	83.1	76.1	68.0
Domestic (6 scales)	91.8	76.9	63.1	51.7
Average difference[a]	.8	6.2	13.0	17.4

[a] Foreign minus domestic.

establishing that these attitudes are available and accessible for use in voting. It is sufficient to demonstrate that these attitudes are available, are as accessible as domestic attitudes, and affect the voting behavior of much of the public.

Campbell's conditions were operationalized using seven-point issue scales (Abramson, Aldrich, and Rohde 1983, 1987; Aldrich and Trump 1986). The conditions were measured cumulatively: the first as the percentage of respondents able to place themselves on a seven-point issue scale, the second as the percentage of respondents who were in addition able to place the two major parties' nominees on the scale, and the third as the percentage of respondents who were in addition able to place the two candidates at different scale positions. The last condition was also evaluated by examining the percentage of respondents who, in addition to satisfying the three preceding criteria, placed the Democrat at a more liberal position than the Republican. Satisfaction of these criteria is evidence that the issue is available and aroused some affect, while the last two measures also indicate satisfaction of the issue voting condition that respondents must believe that one candidate or party better represents their position on an issue.

Table 1 presents data from three data sets on the 1980 and 1984 elections that evaluate these issue voting conditions for both foreign and domestic policies. In each case large proportions of the public satisfied the first criterion: averages of over 80% of the respondents were able to place themselves on each group of seven-point issue scales. With the exception of two items, at least 80% were able to place themselves on each scale.[6] There are no substantial differences between the percentage of respondents able to place themselves on foreign and domestic issues.

There are differences between foreign and domestic issues in the three criteria that involve perceptions of candidate positions. The criteria listed in Table 1 are cumulative, so the last column shows the percentage of respondents in each survey who (1) placed themselves, (2) placed both candidates, (3) perceived differences between both candidates, *and* (4) placed the Democrat at a more liberal position than the Republican. In 1980, for example, over 8% more respondents satisfied the cumulative criteria on the foreign than did so on the domestic issues, while over 17% did so on the 1984 Gallup survey.[7] In each of the three surveys, the largest percentage of respondents satisfied the full set of criteria on the defense spending item. That figure ranges from 54% in the 1980 NES survey to 76% in the 1984 Gallup survey (see Table A-1). Foreign policy attitudes and perceptions *are* as available and appear to be as meaningful as attitudes on domestic issues.

But what about earlier elections? In 1976 the NES survey included *no* foreign or defense policy scale. In 1972 there was one scale about the Vietnam War, and on that item, 73% satisfied all four criteria listed in Table 2, exceeding all of the scales included in the NES surveys from 1972 through 1984.

On all of the issue scales there is a large proportion of respondents who satisfy the

Table 2. The Public's Views of the Most Important Problems Personally and Nationally (%)

Issue Area	Nationally Most Important	Personally Most Important
Foreign & defense		
Soviet Union	9	4
Spending for defense	3	2
Nuclear weapons & the arms race	25	14
Subtotal	37	20
Government & economics		
Taxes	6	16
Budget deficits	13	6
Unemployment & jobs	24	17
Subtotal	43	39
Social issues		
Race discrimination	2	3
Women's rights	1	2
Abortion	3	4
Subtotal	6	9
Government domestic programs		
Pollution and environment	5	8
Social security	5	16
Spending on social services	5	8
Subtotal	15	32
Total	100	100
Number of cases	1,418	1,358
Percentage of total cases (1,509)	94	90

Source: 1984 national survey conducted by Gallup poll for the authors.

issue-voting criteria. Direct comparisons to Campbell and colleagues' (1960) findings are inappropriate because the measuring instruments changed significantly. Still, one suspects that Campbell and associates would have drawn quite a different conclusion had they analyzed seven-point issue scales and found that an average of over half of the respondents met their three criteria on both foreign and domestic issues.

The Accessibility of Foreign and Defense Policy: Most Important Problems

Available political attitudes must also be accessible to have an impact on voting behavior. Most of the public receives its information about candidates and issues from the mass media, which serve as the key priming agent for accessibility. Coverage of events, personalities, and issues serves to heighten the accessibility of political attitudes. Conventional measures of "issue salience" should therefore be conceptually related to the concept of construct accessibility.

To investigate the accessibility of foreign and defense issues, we present data from a number of surveys that asked respondents to indicate what they thought was "the most important problem facing the country today" or, in one instance, "the most important political problem facing them personally."[8] Turning first to the 1984 election, we present results from two surveys. The data in Table 2 are from the Gallup survey noted earlier. Respondents were asked to select, from a list of 12, the most important problem facing the nation. They were also asked which political problem on the list was most important to them personally. Table 3 indicates comparable results from the 1980 and 1984 NES surveys (Abramson, Aldrich, and Rohde 1987).

Table 3. The Public's Views of the Most Important Problem Facing the Nation, 1980 and 1984 (NES) (%)

Issue Area	1980	1984
Foreign and defense		
Foreign	9	17
Defense	8	17
Iran	15	—
All others	—	9
Subtotal	32	34
Government & economics		
Unemployment & recession	10	16
Inflation & prices	33	5
Deficit & government spending	3	19
All others	10	—
Subtotal	56	49
Social issues		
Social welfare	3	9
Public order	1	4
All others	3	—
Subtotal	7	13
Functioning of government	2	2
All others	3	3
Total	100	101
Number of cases	1,352	1,780
Total cases	1,408	1,943
Percentage of total cases	96	93

Source: Adapted from Abramson, Aldrich, and Rohde (1983, 121; 1987, 166).

Over a third of the respondents in both surveys selected foreign and defense issues as the most important problem facing the country. The Gallup data indicate that they were most concerned about nuclear weapons and the arms race—fully one quarter of the respondents picked that issue, greater than any of the other 12 problems. A fifth of the respondents selected a foreign or defense concern as the most important political problem facing them personally, indicating that such problems are *not* remote to many people's lives. Overall, 40% of the public cited foreign or defense issues as most important to the country, to themselves, or to both.

In the 1980 NES survey (Table 3), nearly a third of the respondents cited a foreign or defense issue as the most important problem facing the country. Their concern was heavily focused on the Iranian hostage crisis, although 17% cited another foreign or defense issue. Whether a third is high or low is a relative judgment, of course, but in 1980 the economy was problematic, with double-digit inflation and interest rates. It is, therefore, unsurprising that over half of the public cited economic problems as the most important problem; nor is it surprising that far fewer people did so in 1984, given the vast improvement in the economy. Clearly, concern about foreign and defense affairs was relatively high in both elections. While the target of that concern varied, so did the relative salience and targets of concern on domestic issues.

In 1976, by striking contrast, only 4% cited a foreign or defense issue as the most important national problem (see Abramson, Aldrich, and Rohde 1987). Concern in that year was almost exclusively domestic and, in particular, economic. It is hard to imagine a better single indication of change in attitude accessibility with change in political and economic circumstances. More importantly for our purposes, the accessibility of foreign and defense issues is highly variable. In the 1972 election, for example, nearly a third of the public cited foreign affairs or defense as the most important problem, virtually all of them focusing on Vietnam. Thus, not only is public concern about foreign policy highly variable and systematically related to changing political circumstances but in 1976 this concern was unusually low.

The conclusion that 1976 was atypical is confirmed by an examination of responses to the long-running Gallup most-important-problem question (Smith 1985). From 1947 to the end of U.S. involvement in the Vietnam War in 1973, the smallest proportion of the entire sample selecting a foreign policy issue as *the* most important problem was *never* lower than 22%; the proportion rarely dropped below one-third of all respondents, and it often approached or exceeded the proportion who selected a domestic problem. Even in the period between the Korean and Vietnam Wars, in only 7 of 54 polls did less than 30% of the public select a foreign policy issue. Twenty-one times in this period, this percentage exceeded 50. From the end of World War II to the end of the Vietnam War, foreign affairs was a truly major concern to much of the electorate. It was only somewhat less so in the 1980s, and it was of central concern in both the 1980 and 1984 elections. Conversely, the 1973–79 period stands out as a time when the public was not very concerned about foreign or defense issues. Between February 1973 and January 1980, foreign problems were cited by more than 10% of Gallup's sample only *once* (Smith 1985).

In sum, foreign problems were exceptionally salient to the public throughout the post-World War II era until about 1973. Attitudes on foreign affairs, therefore, were often as or more accessible than attitudes on domestic issues. In the midsixties concern was heavily focused on the Vietnam War, but Vietnam was obviously a *foreign* problem. Vietnam, Korea, Iran, nuclear weapons, and the arms race are of major public concern in ways commensurate with domestic policies. In short, foreign and defense issues are ordinarily highly accessible political concerns. As such, the electorate's foreign policy attitudes are just as accessible during most presidential elections as their attitudes on domestic issues. Moreover, specific international problems, or "hot spots," tend to dominate the public's foreign policy concerns at various times in an ebb and flow not fundamentally different from that characteristic of domestic issues. In 1980, for instance, the most commonly cited concern was inflation or prices. In 1984 inflation was seldom mentioned, but concern about unemployment, recession, and deficit spending increased dramatically (see Table 3). The domestic "hot spots" had shifted.[9]

Candidates and Issues: Perceptions in 1980 and 1984

We have established that the public's foreign policy attitudes are available and are accessible during most campaigns. In addition, much of the public perceives systematic differences between the presidential candidates on foreign policy issues. A remaining question is whether the perceived differences between candidates are as great on foreign issues as they are on domestic issues. Theoretically, even accessible attitudes will affect voting behavior only *if people perceive clear* choices between the candidates.[10] At the extreme—when candidates are perceived to adopt the same position—there is no issue basis for choice. We assess whether the public perceived differences between Reagan and his Democratic opponents that were as great on foreign as on domestic issues.

Perceptions of candidates' positions on issues are subject to rationalization. For example, a liberal may favor a candidate and therefore be likely to perceive that the candidate is also liberal on particularly salient issues (Brody and Page 1972; Page and Brody 1972). For this reason we adopt the procedure used by Page (1978) and examine only the median perception of candidate positions. In using this procedure, we assume that rationalization works in both directions and is averaged out. Moreover, there is no particular reason to believe that rationalization is more likely to occur on foreign than on domestic issues. Rationalization is probably greater for less-informed citizens (Berelson, Lazarsfeld, and McPhee 1954), on less well formed attitudes, and on more-ambiguous candidate positions (Page and Brody 1972). As we have seen, foreign issues were as salient as domestic issues in these two elections, suggesting that rationalization will not differentially affect perceptions on these two sets of issues. Furthermore, on the basis of campaign rhetoric in 1984, it is reasonable to assume that Reagan and Mondale differed considerably in their issue positions on both sets of issues.

Table 4 presents median perceptions on foreign and domestic issue scales for the three surveys. In all three surveys, respondents perceived greater differences between the candidates on foreign and defense issues than on domestic issues. In the 1980 NES data the difference is 1/2 point. In the 1984 NES data the difference between foreign and domestic issues is slight, about 1/4 point. In the 1984 Gallup data the difference is close to a full point. It is striking how far apart Reagan and his opponents were on all of these issues, from an average of over 1½ points (out of *a maximum of 6 points*) on domestic issues in 1980 to nearly 3 points on foreign issues in the 1984 Gallup data. In the latter data set the candidates were at least 2½ points apart on every issue.[11]

Foreign Policy Issues and the Vote

If the claims advanced so far are true, attitudes on foreign affairs should have a significant effect on electoral choice, as great or greater than that of domestic issues.

We have specified a vote choice model that includes the three most proximate "causes" of the vote: attitudes on parties, candidates, and issues. All three surveys include the standard party identification question. Attitudes toward the candidates

Table 4. Average of Median Perceptions of Candidates on Seven-Point Issue Scales, 1980–84

Issue Scale	Reagan	Carter (1980)/ Mondale (1984)	Difference between Candidates
1980 (NES)			
Foreign (2 scales)	5.19[a]	3.09	2.10
Domestic (5 scales)	4.67	3.07	1.60
Difference	—	—	.50
1984 (NES)			
Foreign (3 scales)	5.56	3.39	2.17
Domestic (4 scales)	4.94	3.05	1.89
Difference	—	—	.28
1984 (Gallup)			
Foreign (2 scales)	5.96	3.05	2.91
Domestic (6 scales)	5.05	3.04	2.01
Difference	—	—	.91

[a] These are the average medians for domestic and foreign issues within each survey. For example, medians were computed for all five domestic issues in 1980, and the average of these medians computed and presented.

are measured in the two NES surveys by the difference between perceptions of the two candidates' leadership qualities (with a four-point response scale). In the 1984 Gallup data, the comparable difference measure is based on seven-point like-dislike ratings.[12]

To assess the importance of foreign issues, an international policy scale was created in each data set by summing self-placement scores on the relevant items, six from the 1984 Gallup data (alpha = .66), three from the 1984 NES survey (alpha = .51), and two from the 1980 NES survey (r = .12). There was, of course, greater flexibility in creating domestic scales. In each data set, we created domestic scales with the same number of variables as the international policy scales. We also selected domestic issues that were among those most strongly related to the vote.[13] The dependent variable is whether the respondent reported voting for Reagan or for his Democratic opponent.[14] The equations were estimated using probit analysis.

The results are reported in Table 5. In each instance all four independent variables are strongly related to candidate choice. The overall fit of each model is impressive, with R-squared analogues between .76 and .89, large chi-squared goodness-of-fit measures, substantial percentages of correct predictions (between 78% and 91%), and very strong correlations between actual and probit-predic vote (.73 to .81). As expected, comparative candidate evaluations and partisan identification are very strongly related to the vote, but so too is the overall impact of issues on the vote. In the last column for each data set in Table 5, there is a probit analog to the standardized beta (see Kessel 1988 for details). The joint impact of the two issues is in each case almost as strong as that of either party identification or candidate evaluations.

In 1984 the impact of international issues was slightly greater than that of domestic issues, while in 1980 the opposite was true. In both cases the important point is that in a reasonably fully specified

Table 5. Probit Estimated Vote Equations

Independent Variable	1980 NES Data MLE	1980 NES Data t-ratio	1980 NES Data "Standardized Beta" Analog	1984 NES Data MLE	1984 NES Data t-ratio	1984 NES Data "Standardized Beta" Analog	1984 Gallup Data MLE	1984 Gallup Data t-ratio	1984 Gallup Data "Standardized Beta" Analog
International	-.78 (.037)	-2.13*	-.093	-.107 (.021)	-5.13**	-.184	.068 (.014)	5.02**	.168
Domestic	-.084 (.034)	-2.53**	-.114	-.070 (.023)	-2.99**	-.114	.054 (.014)	3.73**	.133
Party identification	-.359 (.047)	-7.60**	-.367	-.449 (.041)	-11.04**	-.477	.500 (.040)	12.62**	.474
Candidate evaluation	.736[a] (.075)	9.80**	.521[a]	.525[a] (.067)	7.82**	.345[a]	.319[b] (.037)	8.62**	.372[b]
Constant	2.37 (.384)	6.17		3.18 (.343)			-5.33 (.504)	-10.58	
Goodness of fit									
− 2 × LLR[c]		434.9**			771.6**			838.4**	
Pseudo R^2		.76			.89			.84	
Percentage correctly predicted		87			78			91	
Correlation, actual/predicted		.73			.78			.81	

Note: Standard errors are in parentheses.

[a] Based on difference of candidates as "providing strong leadership."

[b] Based on the difference in responses to like-dislike evaluations of the two candidates.

[c] Chi^2, df = 4.

*Significant at .05.
**Significant at .01.

model foreign and defense issues appear to be very important influences on the choices voters make. When taken in combination with the results of the previous sections, the evidence that foreign and international issues were important to the public in 1980 and 1984, were based on available and accessible attitudes and perceptions, and were at least as important as domestic issues in their impact on voting behavior is compelling.

Summary and Conclusions

Our data do not allow us to test the accessibility of foreign policy attitudes directly. We cannot, therefore, establish whether such attitudes are chronically accessible in politics or whether they are only accessible when they are primed by candidates and, more generally, by national election campaigns. One might suspect, however, that such attitudes are chronically accessible and that the chronic accessibility of such attitudes is enhanced by campaign priming. First, peace stands with prosperity as an outstanding example of enduring goals held by the public (see Ostrom and Simon 1985). Second, with the exception of the period between 1973 and 1980, foreign issues have been commonly cited as among the most important problems facing the nation. Third, the importance of Vietnam in 1972 parallels the importance of foreign and defense affairs in 1980 and 1984. There was a decreased number of citations of foreign and defense problems in 1976, but data are not available to determine whether there was also a decline in the structure or impact of foreign policy attitudes. We can conclude that in the elections for which we have appropriate data, attitudes about foreign affairs have been consequential. The resolution of the anomaly is simply that candidates do *not* waltz before a blind audience. Ironically, it appears that the only blind audience has been a significant portion of the scholarly community.

That candidates do campaign on foreign policy themes is itself a source of attitude accessibility for much of the public. Campaigns may temporarily activate foreign policy attitudes, although they may not change many minds. Indeed, the stronger the attitudes, the more immune they are to change. Further, priming or other means of attitude activation, have been shown to activate other, closely related constructs. If, therefore, candidates emphasize or discuss a particular foreign policy issue, attitudes about other foreign policy issues may also be activated. The candidates' campaign strategies may help, in part, to ensure a potent role in electoral choice for attitudes about foreign policies.

This argument suggests that candidates can manipulate the degree to which particular attitudes are activated. Greater discussion and coverage should accentuate—while lesser discussion will reduce—the accessibility of foreign policy attitudes. Of course, external events, news media, and the competition—as well as individual differences in members of the public—may overshadow a candidate's efforts. Elections vary in the salience of foreign policy issues, and candidates can, within limits, address these issues to create greater or lesser differences between themselves. Accordingly, we have created a fourfold election typology by examining two dimensions: the degree of emphasis on foreign and defense policies, and the differences between the candidates on these issues. The greater the foreign-defense attitude accessibility and the greater the distinctiveness of the candidates, the more important foreign and defense issues will be in voter choice.

The data suggest that the 1972 and 1984 elections maximized both the accessibility of foreign policy attitudes and the differences between the candidates on these issues. Foreign policy attitudes were also

Figure 1. A Typology of Elections and Foreign Policy Issues

	Small Difference Between Candidate Stances	Large Difference Between Candidate Stances
Low Salience and Accessibility	Low Effect of Foreign Issues 1976	Low to Some Effect 1956?
High Salience and Accessibility	Low to Some Effect 1968 1960?	Large Effect 1972 1952? 1980 1964? 1984

important determinants of the vote. The 1980 election was only slightly below these two elections on both criteria. In 1968 the accessibility of attitudes on Vietnam was high, but candidate differences were low. As a result, that issue was not so important in explaining voting behavior. In the absence of appropriate data, the indirect evidence reviewed above suggests the hypothesis that in 1976 accessibility of foreign policy attitudes was very low and candidate differences were not especially sharp. These attitudes, therefore, probably played only a minor role in voting. The 1960 election was one in which accessibility was probably high (Smith 1985), and, assuming that differences between Nixon and Kennedy were not particularly great, this election was probably more similar to that of 1968 than to that of either 1972 or 1984. The 1952 and 1964 elections, conversely, were probably similar to those of 1980 and 1984, with large differences between the candidates and relatively high salience. The closest possible example of a relatively high-candidate-difference, low-accessibility election might be 1956. After all, the two candidates were the same as in 1952; but with the end of the Korean War and McCarthyism the role of foreign affairs was probably reduced from 1952.[15] These speculations are summarized in Figure 1; and if this election classification is correct, foreign and defense attitudes have had a strong impact on voting in most of the recent elections and have played at least some role in virtually all presidential elections during the previous thirty-odd years.

Thus the anomaly—why candidates often campaign on foreign policy issues when so many scholars claim that the public does not respond to these appeals—has been resolved. The candidates are acting reasonably, because voters do in fact respond to their appeals. The candidates are waltzing before a reasonably alert audience that appreciates their grace. And, given a choice, the public votes for the candidate who waltzes best.

Appendix

Table A-1 presents scale-by-scale the data for the averages in Table 1.

Table A-1. Percentages Meeting Various "Issue Voting" Criteria, 1980–84

Issue Scale	Placed Self on Scale	Placed Self & Both Candidates	Placed Self & Saw Difference between Candidates	Placed Self & Saw Democrat As More Liberal Than Republican
1980 (NES)				
Foreign				
Defense spending	85.1	69.5	63.5	54.1
Russian relations	84.5	70.8	58.8	51.0
Domestic				
Government services & spending[a]	81.5	64.2	56.8	46.8
Inflation and unemployment	59.5	46.5	38.3	26.7
Aid to minorities	85.9	69.1	61.1	52.8
Women's rights	93.6	72.2	54.4	46.9
Jobs & standard of living	84.2	68.3	57.4	48.6
1984 (NES)				
Foreign				
Central America[a]	77.7	64.5	55.2	45.5
Defense spending	88.0	77.9	71.3	65.2
Russian relations	85.3	73.7	62.0	52.9
Domestic				
Government services & spending[a]	85.5	76.2	69.7	60.0
Aid to minorities	87.9	76.5	62.9	49.6
Aid to women	85.2	74.0	59.8	49.4
Jobs & standard of living	87.1	76.0	60.7	53.6
1984 (Gallup)				
Foreign				
Strong or tough stance[a]	91.9	80.6	71.1	60.8
Defense spending	93.4	85.5	81.2	76.2
Domestic				
Government services & spending[a]	89.6	80.4	76.4	66.9
Affirmative action	92.9	74.7	61.2	50.3
Government & morals[a]	90.0	73.5	53.4	39.0
Aid to minorities	92.9	78.0	65.2	53.7
Jobs & standard of living	93.7	79.5	65.9	58.0
Government & religion[a]	91.7	75.1	56.4	42.5

[a] Denotes scales with scoring reversed so that 1 = most liberal and 7 = most conservative.

Table A-2 presents scale-by-scale the data for the averages in Table 4.

Table A-2. Median Self-Placement and Perceptions of Candidates on Seven-Point Issue Scales, 1980–84

Issue Scale	Self	Reagan	Carter and Mondale
1980 (NES)			
Foreign			
Defense spending	5.37	5.76	3.70
Russian relations	3.93	4.62	2.48
Domestic			
Government services & spending[a]	3.68	4.68	2.89
Inflation & unemployment[a]	3.92	4.36	3.65
Aid to minorities	4.46	5.00	3.01
Womens' rights	2.40	4.28	2.67
Jobs & standard of living	4.43	5.04	3.14
1984 (NES)			
Foreign			
Central America[a]	3.24	5.51	3.53
Defense spending	4.05	5.96	3.32
Russian relations	4.16	5.22	3.32
Domestic			
Government services & spending[a]	3.97	5.41	2.86
Aid to minorities	4.09	4.63	3.07
Aid to women	3.88	4.58	3.02
Jobs & standard of living	4.23	5.14	3.24
1984 (Gallup)			
Foreign			
Strong or tough stance[a]	4.46	5.86	3.24
Defense spending	3.83	6.06	2.86
Domestic			
Government services & spending[a]	3.72	5.41	2.52
Affirmative action	6.13	5.34	3.28
Government & morals[a]	2.43	4.44	3.39
Aid to minorities	4.13	4.91	2.82
Jobs & standard of living	4.86	5.71	3.25
Government & religion	2.29	4.48	2.98

[a] Denotes scales with scoring reversed so that 1 = most liberal and 7 = most conservative.

Notes

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1. One could also argue that presidential candidates emphasize foreign affairs to appeal to small but important groups and leaders. Here we seek to demonstrate that the presidential campaign appeals to the general public, as well as to key groups or leaders.

2. He paid particular attention to the Panama Canal Treaty and later to détente and Kissinger's stewardship of foreign policy. John Sears made this strategic decision, and Reagan concurred because of the emotional and supportive audience reaction to mention of the treaty.

3. Very few citizens relished the details of debates over Quemoy and Matsu in 1960, the B-1 bomber or MX missile debates of more recent vintage, or Reagan's SDI plan. Many of them were interested, however, in the *general* issues involved in SDI. Concern over nuclear weapons and the arms race, defense spending, and relations with Russia were indeed important concerns in 1984 (Table 3). Just as many people have general views about inflation and unemployment but know little about the technical details of fiscal and monetary policy, so also do they have general views about foreign policy without detailed knowledge.

4. The survey was conducted for the authors between 9 and 15 November 1984. We interviewed a total of 1,509 respondents, using standard Gallup procedures.

5. There were no foreign or defense issue scales included in the 1976 NES survey, while in 1972 there was only the Vietnam War scale.

6. All three surveys include more domestic than foreign policy items. It might therefore be that two or three domestic items meet the conditions much better than the foreign affairs items but that this information is lost in the averaging process. We have presented the scale-by-scale data in the Appendix (Table A-1).

The averages do not distort the data. The differences between the Gallup and NES surveys are not due to differences in screening procedures, since they were identical.

7. The items that measure defense spending, government services, jobs, and aid to minorities were worded identically in the 1984 NES and Gallup surveys.

8. The question arises whether this "importance" operationalization captures the accessibility of the issue. Recent data from Fazio (personal communication) show that what people indicate to be the most important social issue may indeed be the most accessible as measured with reaction time data. In addition, there are data suggesting that the accessibility of an issue is related to the accessibility of one's *attitude* towards that issue.

9. The NES data reported in Table 3 report only the percentages selecting an issue as the single most important problem. Examining the fuller array shows that somewhat larger percentages cited foreign and defense concerns as *one* of the most important problems. Thus our measurement can underestimate, but not overestimate, issue accessibility.

10. Perceiving differences between the candidates is a necessary but not sufficient condition for issue voting, because moderate voters may be indifferent between a very liberal and a very conservative candidate. In the aggregate, however, the greater the differences between the perceived positions of the candidates, the larger the proportion of voters who will *not* be indifferent.

11. The medians are presented separately for each issue in the Appendix, Table A-2. There is a clear pattern.

There are large interelection differences on domestic issues. In 1976, the candidates were perceived to be much less distinct on domestic issues than in the 1980 and 1984 elections, virtually issue by issue. In 1972 McGovern was seen to be very liberal on nearly every issue while Nixon was perceived to be moderately conservative on most issues, producing differences that were actually slightly smaller than in 1984. The 1972 NES data included the Vietnam issue, upon which the public saw differences between the two candidates at least as great as those perceived on most domestic issues (Aldrich and McKelvey 1977; Miller et al. 1976).

12. This equation is well specified only in the sense of controlling for other proximate factors in the vote equation. Furthermore, both sets of issue measures would be more strongly related to voter choice if we had relied on measures of the voters' proximity to their perceptions of the candidates' positions on these issues (Rahn et al. n.d.). We did not do so, however, wishing to minimize rationalization. They would also show a stronger relationship if we included retrospective measures, such as Carter's handling of the Iranian crisis.

13. The six issues in the 1984 Gallup data were government services versus spending, the impact of recent government programs on minorities, budget deficit reduction, women's role, aid to minorities, and jobs and standard of living (alpha = .66). The three issues from the 1984 NES survey were government services versus spending, aid to minorities, and jobs and standard of living (alpha = .59). The last two were also used to create the domestic policy scales in the 1980 data ($r = .43$). Since the reliabil-

ities are at least as high on the domestic scales as on the international scales, measurement error attenuates the relationship between voting and international issues at least as much as that between voting and domestic issues.

14. Therefore, those who reported abstaining or voting for some other candidate—like John Anderson—are excluded.

15. Kessel's (1988) results on the salience and impact of international involvement are broadly consistent with these speculations, even for 1976 and 1968. The 1960 election is an exception because he found a large impact on the vote.

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John H. Aldrich is Professor of Political Science, Duke University, Durham, NC 27706.

John L. Sullivan is Professor of Political Science and Eugene Borgida is Professor of Psychology, University of Minnesota, Minneapolis, MN 55455.

THE NATURE OF UTILITY FUNCTIONS IN MASS PUBLICS

HENRY E. BRADY
University of Chicago

STEPHEN ANSOLABEHERE
Harvard University

Survey data are used to test the basic tenets of rational choice theory. We find that (1) voters' preferences are orderly but voters do not always have preferences. When the perceived difference between two candidates is below some threshold, voters are indifferent between them. (2) Most voters choose candidates whose issue positions are congruent with their own. (3) Lack of knowledge about candidates affects choice and utility through two routes—indifference and risk aversion.

Most voters have highly structured utility functions, but the thresholds and risk aversion in these functions place relatively unknown candidates at a disadvantage in presidential primaries and other electoral situations. Moreover, the importance of knowledge in these functions suggests that social choice theorists must take seriously the classic concerns of public opinion research—the operation of the media and the dynamics of information in elections.

In the last 20 years rational choice theory has changed the way political scientists think about voting behavior. A central tenet of this approach is that people's preferences can be characterized by a utility function. Weaker assumptions are sometimes made in social choice theory (e.g., Schwartz 1986), but utility functions are a mainstay of most public choice theorizing (e.g., Ordeshook 1986, 12–13). Despite the importance of this assumption, few political scientists, if any, have tried to test it directly.[1] We do just that using survey data on voters' preferences as to candidates for the presidential nomination.

We go even further, however. By many accounts, rationality not only turns on the consistency of preferences, it also turns on their content. In their workaday model building, public choice theorists and economists often justify "each act in terms of self-interest" (Sen 1977, 342). The paradoxes of participation (Olson 1965) and voting (Riker and Ordeshook 1968), for example, stem from this assumption. Self-interest is a tricky notion, but we shall subject it to some crude tests. In particular, we examine the extent to which preferences are grounded on political ideologies and issues.

Finally, in the models of preferences developed here, we show how knowledge and information enter in surprising ways. As a result, social choice in presidential primaries may be greatly affected by how information is generated by the media and processed by individuals. This suggests that students of public opinion and voting should pay special attention to the operation of the media and the dynamics of information.

Why does rational choice theory assume that utility functions exist? There are at least two basic reasons. The first revolves around measurement considerations.[2] Even though public choice theorists seldom try to measure preferences directly, their models are simplified if preferences can be easily quantified and analyzed with simple mathematical tools. Utility functions provide an easy way to handle preferences in formal models. The second reason revolves around normative considerations. People whose preferences cannot be represented by a utility function can be continually bilked out of their money by means of clever trades that take advantage of their inconsistencies. They can be exploited as "money pumps."[3] This kind of behavior seems irrational on its face, so most theorists would like to rule it out by assuming that utility functions exist.

If utility functions exist, preferences must have an internal structure that constrains the permissible combinations of choices. Two assumptions are necessary for the standard kind of utility function. First, preferences must be transitive. If an individual prefers *j* to *k* and *k* to *m* then the individual must prefer *j* to *m*. Second, indifference must be transitive. If an individual is indifferent between the pair *j* and *k* and between the pair *k* and *m*, then the person must be indifferent between *j* and *m*. The failure of the second assumption is one of our central themes. Some voters are not perfect discriminators. Their preferences are transitive, but their indifference is structured by weaker properties than transitivity. They have thresholds below which they are indifferent between two candidates and above which they can make clear-cut discriminations.

If preferences have any meaning, they must be related to people's interests. They must be related in a comprehensible way to salient features of the alternatives and to people's political knowledge, opinions, and perceptions (Thompson 1970, chap. 5). We find, in fact, that preference and indifference are related to the political opinions of voters, their knowledge about the candidates, and the characteristics of the candidates. Preferences can be related to a dimensional representation of candidate characteristics, but knowledge about the candidates also plays an important role in choice. For example, a lack of knowledge about a candidate interacts with a voter's risk-aversion to decrease the likelihood that he or she will prefer the candidate to other candidates. And uncertainty about candidates makes it harder for the voter to discriminate between them. These results are more than curious facts about individual voting behavior. They have implications for theories of rational behavior and for the dynamics of presidential primaries.

We will first review the axiomatic approach to rational choice theory. Then we will investigate the structure of individual choices by seeing how closely they conform to these axioms. We will investigate the meaning of pairwise comparisons by relating them to data on individual knowledge and opinions, paying special attention to how information affects voting choices. Finally, we will discuss what our results imply for individual choice theory and for voting in presidential primaries.

An Axiomatic Treatment of Rational Choice

Axiomatic theories of preference developed in economics and decision theory are based upon binary relations that express the relationships between all the pairs taken from a set of objects.[4] For example, if *J* represents a set of candidates $\{j,k,l, \ldots, m,n\}$, then $j\ P\ k$ means the person prefers *j* to *k*, $j\ I\ k$ means the person is indifferent between *j* and *k*, and $j\ R\ k$ means the person either prefers *j* to *k* or is indifferent between them. These relation-

Table 1. Rational Choice Orderings

Type of Ordering	Axioms	Numerical Representation
Linear	irreflexive, transitive, weakly connected	$U(j) > U(k)$ iff $j\ P\ k$ $U(j) = U(k)$ iff j is k
Weak	asymmetric, negatively transitive	$U(j) > U(k)$ iff $j\ P\ k$ $U(j) = U(k)$ iff $j\ I\ k$
Semi-	irreflexive, equal thresholds, threshold existence	$U(j) - U(k) > V$ iff $j\ P\ k$ $U(k) - U(j) > V$ iff $k\ P\ j$ otherwise $j\ I\ k$
Interval	irreflexive, threshold existence	$U(j) - U(k) > V(k)$ iff $j\ P\ k$ $U(k) - U(j) > V(j)$ iff $k\ P\ j$ otherwise $j\ I\ k$
Partial	irreflexive, transitive	$U(j) > U(k), \ldots, W(j) > W(k)$ iff $j\ P\ k$ otherwise $j\ I\ k$
Sub-	transitive closure	$j\ P\ k$ implies $U(j) > U(k)$ but not the reverse

ships can be characterized by axioms that fall into three categories: "definitional," "rationality," and "completeness" axioms:

definitional axioms

reflexive: $j\ R\ j$
irreflexive: not $j\ P\ j$
asymmetric: $j\ P\ k$ implies not $k\ P\ j$
symmetric: $j\ I\ k$ implies $k\ I\ j$

rationality axioms

transitive closure: $j\ P\ k$, $k\ P\ l, \ldots, m\ P\ n$ implies not $n\ P\ j$
transitive: $j\ P\ k$ and $k\ P\ n$ implies $j\ P\ n$
threshold existence: $j\ P\ k$, $k\ I\ m$, and $m\ P\ n$ implies $j\ P\ n$
equal threshold: $j\ I\ k$, $k\ P\ m$, and $m\ P\ n$ implies $j\ P\ n$
negatively transitive: not $j\ P\ k$ and not $k\ P\ n$ implies not $j\ P\ n$

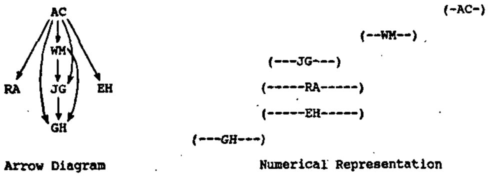
completeness axioms

weakly connected: j not equal to k implies either $j\ P\ k$ or $k\ P\ j$

Fishburn (1970c, 1972) provides in-depth discussions of these axioms. The "definitional" properties, as the name suggests, are used to define each type of relationship, and we shall always assume that P is asymmetric, R is reflexive, and I is reflexive and symmetric. We shall also assume that $j\ I\ k$ if and only if not $j\ P\ k$ and not $k\ P\ j$.

The "rationality" properties add substantial additional content to our understanding, and they are usually thought of as the basic requirements for truly rational choice (Herzberger 1973; Ordeshook 1986, 14). When considered in conjunction with the asymmetry assumption for strict preference, these properties define a series of increasingly stronger "orderings" of a set of candidates. Table 1 lists the axioms for each of these orderings, from the strongest to the weakest, along with some of their properties. One way to characterize these orders is to show how they can be related to the properties of the real number system. For example, the "weak order" in Table 1 can be described by a one-dimensional utility function that assigns a real number to each candidate (Fishburn 1970c, Theorem 2.5). The ordinal properties of these num-

Figure 1. Interval-Ordered Case: Self-administered Questionnaire Respondent Number 9



Arrow Diagram

Numerical Representation

Note: Acronyms are initials of Reubin Askew, Alan Cranston, John Glenn, Gary Hart, Ernest Hollings, and Walter Mondale.

bers represent the properties of the weak ordering.[5]

Utility functions provide simple representations of preferences, but there is no a priori reason why people must have utility functions. Human beings were not designed for the convenience of the model builder or the empirical researcher. People might, for example, have only transitive preferences, that is, a partial ordering. In this case an individual reasons consistently about objects he or she can compare, but many choices are incomparable. There is no simple, unidimensional summary or utility. Rather, one needs a multidimensional numerical representation to describe a person's reasoning. Unfortunately, these representations are not a very tidy summary of people's preferences (Roberts 1972).

The utility functions listed in Table 1 offer a way to unify our understanding of preference orderings. At a minimum we hope people exhibit transitivity in their preferences. Then at least a partial ordering characterizes choice. If there is some structure to indifference as well as preference, we can characterize choice with just two functions—utility and an indifference threshold. The existence of a threshold means that it may be impossible to distinguish fairly similar alternatives. But it is easy to differentiate between fairly distant alternatives. W. E. Armstrong first proposed this idea in a series of papers published between 1939 and 1950. For Armstrong, "the non-transitiveness of indifference must be recognized and explained on any theory of choice, and the only explanation that seems to work is based on the imperfect powers of discrimination of the human mind whereby inequalities become recognizable only when of sufficient magnitude" (1950, 122).

The threshold existence property (sometimes called PIP transitivity) allows us to define orderings by the size and variability of the threshold. Asymmetric preferences that also satisfy the threshold existence axiom form an interval order. The rationale for the threshold existence property is probably most clearly seen in the representation theorem for this order.[6] Each candidate j can be represented by an underlying utility $U(j)$ and a threshold $V(j)$ surrounding this evaluation so that j is preferred to k only when

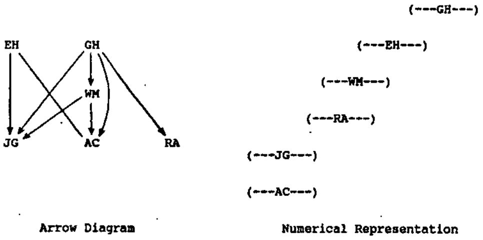
$$j \, P \, k \text{ if and only if } U(j) > U(k) + V(k);$$

that is, j will be preferred to k only if the utility difference between j and k is greater than the threshold value for k. If this inequality does not hold, then the respondent cannot be sure the true utility for j is greater than that for k so the response will be indifference.

An interval order places more structure on indifference than a partial order, and the result is a one-dimensional utility function along with a set of threshold values for each object. Figure 1 presents an arrow diagram for an interval order and a graph of a numerical representation. The convention in the arrow diagram is to represent preference (P) by an arrow and indifference (I) by the absence of an arrow. In the graph on the right of the figure, the leftmost point of each interval is the utility value for the candidate and the length of the interval represents the magnitude of the threshold. For this woman, the variation in the thresholds from candidate to candidate is closely related to her knowledge of the candidates: she reported knowing "nothing at all" about Hollings and Askew, "a little" about Hart and Glenn, "some" about Mondale, and "a lot" about Cranston. We will show that this relationship between knowledge and thresholds is no accident.

Adding the equal thresholds requirement to the axioms for an interval order produces a semiorder. For a semiorder, all of the thresholds $V(j), \ldots, V(m)$ are equal (Fishburn 1970c, 22). Figure 2 presents an example along with a graphical representation like that in Figure 1. All of the thresholds are constant in this case.

Figure 2. Semiordered Case: Self-administered Questionnaire Respondent Number 1



Arrow Diagram

Numerical Representation

Note: Acronyms are initials of Reubin Askew, Alan Cranston, John Glenn, Gary Hart, Ernest Hollings, and Walter Mondale.

A weak order results when an asymmetric relationship also satisfies negative transitivity. These conditions ensure transitive indifference as well as transitive preference. The weak order is the usual model of individual preference, and the ordinal utility function it implies is the usual representation of preferences. A still stronger order is possible in which there is no indifference between any two distinct elements. This linear order assumes that every pair can be distinguished so that a choice can be made.

There is endless debate over which, if any, of these orderings is necessary or sufficient for rational choice. Most rational choice theorists consider transitive preferences and indifference—or the existence of a weak order—the minimal requirement for rationality. Typically two reasons are given: parsimony and consistency of interests.[7] All else equal, there is great virtue in the simple representation of preferences provided by the one-dimensional utility function for a weak order. Parsimony, however, is a weak foundation upon which to construct a theory of rational choice.

Consistency of interests provides a firmer base. Surely people avoid taking positions that might force them to act against their interests. Yet people who have intransitive preferences or intransitive indifference can be treated as money pumps and bilked out of their money. Hence, people must have transitive preferences and transitive indifference.

The power of this syllogism flows from its premise that people abhor acting against their interests, but it seems more powerful when used to reject intransitive preference than intransitive indifference. When stating a preference, a person makes a clear commitment in favor of one thing over another. Presumably the person knows his or her interests in this case. When indicating indifference a person may be making an equal commitment to each alternative or indicating insufficient knowledge to make a commitment. Indifference may indicate equal preference or it may indicate no preference. Intransitive preferences, then, suggest a deep-seated confusion about one's interests whereas intransitive indifference may only indicate temporary ignorance about one's interests.

If intransitive indifference is merely ignorance about one's interests, then it seems less problematic for a theory of rationality than intransitive preferences. As people's uncertainty vanishes, so does their inconsistency. Nevertheless, we might still expect rational indifference to exhibit some structure. Interval and semiorders provide a reasonable sort of structure. Moreover, these orders have convenient representations that require knowing only two facts about each object: its utility and its threshold. Of course, a weak or a linear order imposes even more structure and requires even less information about each object. These observations provide a framework for an empirical study of rationality.

How Rational are Preferences for Presidential Candidates?

We investigate the degree of rationality in two different data sets by using some computer programs that check for the various kinds of orders we have described. These programs take as basic data the matrix Q composed of entries q_{jk} which equal one if j is preferred to k and zero otherwise. For each type of ordering Q has some unique properties, and these have been used as the basis for a set of FORTRAN programs that check for each type of order.

The data sets are a California poll (CP) of a random sample of 1,034 adults completed during March 19-31, 1976 and a March 1983 self-administered questionnaire (SAQ) conducted by Brady involving 170 undergraduate students, graduate students, and faculty and staff of the University of California, Berkeley. Appendix A contains a more complete description of these two surveys. Four data sets were constructed from these two surveys. CP-6 consists of the 307 respondents to the California poll who answered a complete set of 15 paired comparisons for the six candidates Jerry Brown, Jimmy Carter, Gerald Ford, Hubert Humphrey, Henry Jackson, and Morris Udall. CP-5 consists of the full 611 respondents but with the data for Ford omitted. SAQ-I is based on the initial paired comparisons from the SAQ, and SAQ-F is comprised of the final paired comparisons from the SAQ after respondents were given a chance to revise their answers. For every paired comparison we assumed that a failure to choose one or the other of the candidates indicated indifference so that for every pair of candidates j and k we had information on whether $j\ P\ k$, $k\ P\ j$, or $j\ I\ k$.

Table 2 summarizes the kinds of orderings we found. The most remarkable feature of this table is the significant amount of structure found in all of the data sets. Over 60% of the respondents in every data set had weak or linear orders. No more than 20.5% of the respondents had less than partially ordered preferences. Only one person was partially ordered. And from 12.6% to 24.8% of the respondents had semi- or interval orders. These facts suggest that people's preferences satisfy the requirements of rational choice and that their preference can be represented in a convenient form.

Yet we do not want to be too hasty in our conclusions. Is it possible that one would observe these results for a random set of preferences generated by the observed marginal frequencies for each paired comparison? To test for the possibility that our respondents did no more than respond to the paired comparisons independently of one another, we ran 10 simulations of 170 respondents in which each paired comparison response was obtained from a trinomial whose probabilities matched the frequencies with which we observed $j\ P\ k$, $k\ P\ j$, and $j\ I\ k$ in the initial SAQ choices. The average number of each kind of order and the standard deviations of these numbers across the 10 runs are reported in the final column of Table 2. It is obvious that the data for SAQ-I were not generated from this random distribution. This finding is so strong that we did not bother to run further simulations for the other data.

Two more general features of Table 2

Table 2. Respondents with Each Type of Ordering (%)

Type of Ordering	California Poll for March 1976		Self-administered Questionnaire (SAQ)		Simulation of SAQ, Assuming Random Responses
			Initial	Final	
Number of candidates	6	5	6	6	6
Linearly ordered	42.0 (129)	52.2 (319)	10.0 (17)	11.2 (19)	2.2 (.6)
Weakly ordered	18.6 (57)	28.5 (174)	50.0 (85)	58.8 (100)	5.3 (.9)
Semi-ordered	14.7 (45)	11.6 (71)	22.4 (38)	20.0 (34)	2.5 (1.0)
Interval ordered	4.2 (13)	1.0 (6)	2.4 (4)	2.9 (5)	1.2 (.9)
Partial ordered	.0 (0)	.0 (0)	.6 (1)	.6 (1)	1.4 (.7)
Subordered	10.1 (31)	3.9 (24)	7.1 (12)	4.7 (8)	30.2 (1.8)
Intransitive	10.4 (32)	2.8 (17)	7.6 (13)	1.8 (3)	57.2 (2.2)
Total	100.0 (307)	100.0 (611)	100.1 (170)	100.0 (170)	100.0

Note: Numbers in parentheses for first four columns are number of cases in that category. The numbers in parentheses in the last column are standard deviations across the 10 simulations.

deserve comment. First of all, there are more linear orderings with the California poll data than with the SAQ data. One possible explanation for this is that the SAQ experiment actively solicited answers of indifference while the CP survey did not. Another is that the CP was completed in the middle of the primary season when knowledge of the candidates was presumably fairly high whereas the SAQ was completed almost a year before the primaries had begun. Second, there appear to be more intransitivities (cyclic and subordered respondents) as the number of candidates increases. This is very marked in comparing CP-5 with CP-6. This might be due to the fact that the question about Ford (the sixth candidate) was asked in another part of the California Poll. People may have simply forgotten their earlier answers.

The most interesting aspect of Table 2 is the contrast between SAQ-I and SAQ-F. Given a chance, many of our respondents moved from the two bottom "irrational" categories to the four "rational" ones at the top of chart. This is especially pronounced in the cyclic category where 10 of the original 13 respondents moved upwards, and 9 of these people had at least semiorders in the SAQ-F data. Of the 12 subordered people in the SAQ-I data, 6 became at least semiordered. One person moved up to the subordered category from being cyclic, and one person moved down from being interval-ordered to being subordered. The net result was fewer subordered respondents in the final data. There was an approximate steady state in the semi- and interval orders, and the weak orders picked up most of the movers. It appears as if people try to cor-

Table 3. Number of Changes in Preferences by Type of Ordering for Respondents to the Self-administered Questionnaire

	Type of Ordering					
Number of Changes	Cyclic	Suborder	Interval	Semi-	Weak	Linear
0	3	6	3	26	82	15
1	3	1	1	11	2	0
2	4	0	0	0	1	1
3 or more	3	5	1	1	0	1
Percentage with one or more changes	81	67	40	32	4	12

rect intransitive preferences, especially when they involve an actual contradiction.

This impression is confirmed by Table 3, which shows the strong relationship between the number of changes made by the type ordering. Only about 5% of those with linear and weak orders made any changes; 33% of those with semi- and interval orders made changes; and 64% of those with suborders or cyclic preferences made changes. Our respondents seemed satisfied with linear or weak orders, slightly uneasy about semi- or interval orders, and very uneasy about suborders or cyclic preferences. Another measure of their uneasiness and persistence is the number of changes they made once they decided to make a change. Here we find that whereas the median number of changes among those with interval orders or better who made a change was one (of the 19 who changed, 14 made only one change); the median number of changes among those with cyclic preferences or suborders who changed was between two and three. In short, our respondents worked hard to produce at least interval-ordered preferences.

Nevertheless, people did have different types of orders over these candidates. Why do some people have suborders while others have linear orders? One plausible explanation is that some people have greater knowledge than others. Rationality may be linked to level of knowledge and information. But what type of knowledge is relevant to rational political behavior? One obvious answer is that political knowledge is the most important kind of information for political behavior. We tested this notion using two different measures of political knowledge: self-report knowledge on a large number of candidates and willingness to place candidates on a set of political issues. A different answer is that knowledge about how to think clearly is what really counts. The best available measure of this kind of knowledge is the level of education completed by the respondent.

All of these measures had a slightly positive relationship with the type of ordering, but the typical value of Kendall's τ_b was only about .10. Knowledge does not seem to have much to do with the type of ordering. This suggests that rationality is more basic than knowledge. Even those with very little knowledge still demonstrated a fairly high degree of rationality.

Explaining Political Preferences

A Model of Responses to Paired Comparisons

Most people's preferences are structured, and their preferences conform to axioms for an interval order that can be represented by a utility function and a threshold function over the candidates.

Nature of Utility Functions

What is behind these findings? Do paired comparisons provide meaningful results in terms of the political opinions and social and economic characteristics of the individuals who were surveyed? How does knowledge enter into people's preferences? We answer these questions by developing and estimating a model of responses to paired comparisons.

Assume that an individual voter i has an additively separable quadratic utility function for each candidate j:

$$U_{ij} = \sum_{t=1}^{T} (\alpha_{it}X_{tij} + \beta_{it}X_{tij}^2) \tag{1}$$

where the X_{tij} are individual i's perceptions of candidate j's characteristics along dimension t, and where α and β are "taste" parameters. Assume, because of the individual's uncertainty about the true characteristics of each candidate, that the X_{tij} are random variables with finite means μ_{tij} and variances σ_{tij}^2. Then the expected value of equation 1 is as follows:

$$E(U_{ij}) = \Sigma_t(\alpha_{it}\mu_{tij} + \beta_{it}\mu_{tij}^2 + \beta_{it}\sigma_{tij}^2). \tag{2}$$

The functional form in equation 1 was introduced by James Tobin (1958), and it has the great advantage of having the simple expectation in equation 2 because it only contains linear and quadratic terms. Moreover, it allows for risk-taking or risk-averting behavior for each dimension depending upon whether β_{it} is positive or negative.

The obvious competitor is the commonly used Euclidean distance function (Enelow and Hinich 1984), which seems more realistic in the political context:

$$U_{ij} = -\sum_{t=1}^{T} (\alpha_{it} - \beta_{it}X_{tij})^2 \tag{3}$$

In this formulation, the α_{it} are the coordinates of ideal points for each voter. Somewhat surprisingly, equation 3 leads to a result for $E(U_{ij}) - E(U_{ik})$ that is very similar to that obtained from the additively separable quadratic function. The only differences are the constraints on the estimated parameters such as the coefficients on the last and next-to-last term in equation 2. The most bothersome of these constraints is the requirement for a negative risk parameter when the expectation of equation 3 is taken under the assumption that the X_{tij} are random variables. This constraint requires voters to be risk-averters, and it leads us to prefer equation 1. Because we do not impose any of the constraints that follow from equation 1 in our estimation, our procedure includes equation 2 as a special case.

We do not have any usable measures of the respondents' perceptions μ_{tij} of the candidates in equation 2,[8] but we can write each individual's expected utility in terms of the average μ_{tj} of μ_{tij} and a residual component. Furthermore, it seems reasonable to assume that the variation in β_{it} can be split up in the same way into a component β_t that is constant across the population and a residual component. All of these residual components can then be placed in a term ϵ_{ij}:

$$E(U_{ij}) = \Sigma_t(\alpha_{it}\mu_{tj} + \beta_t\mu_{tj}^2 + \beta_t\sigma_{tij}^2) + \epsilon_{ij}. \tag{4}$$

Further simplification is possible by assuming that person i's total uncertainty about candidate j, V_{ij}, is proportional to $\Sigma_t\beta_t\sigma_{tij}^2$, and this can be measured by using some measure of the person's knowledge of each candidate. We have two different measures available that differ in their degree of subjectivity. The first measure based upon self-reported knowledge about the candidates is more subjective than the second constructed from respondents' willingness to place candidates on a set of political issues. We have self-report measures of knowledge available for both the SAQ and CP data, and we have a willingness to place measure for the SAQ data.

We do not have any direct measures of even the average perceptions μ_{tj} of the candidates, but we can write the "tastes" α_{it} in terms of the observable characteristics Y_{is} of the individuals and some unknown parameters γ_{st}:

$$\alpha_{it} = \sum_{s=1}^{S} Y_{is}\gamma_{st}. \tag{5}$$

If this expression is substituted into equation 2 along with V_{ij} (where δ is the constant of proportionality mentioned above), we obtain

$$E(U_{ij}) = \Sigma_s Y_{is}\psi_{sj} + \psi_{0j} + \delta V_{ij} + \epsilon_{ij}, \tag{6}$$

where

$$\psi_{sj} = \Sigma_t \gamma_{st}\mu_{tj} \text{ and } \psi_{0j} = \beta_t \mu_{tj}^2. \tag{7}$$

Equation 6 is what we might have obtained had we begun by simply thinking of individual i's utility for candidate j as a linear function of the person's S observable characteristics (Y_{is}, $s = 1, \ldots, S$) and the individual's uncertainty (V_{ij}) about the candidate. It amounts to the assertion that people with different issue positions or socioeconomic characteristics will evaluate candidates differently and that people who are more uncertain about a candidate will reduce their expected utility for that candidate if they are risk averters (δ is negative) and increase it if they are risk lovers (δ is positive). The ψ_{sj} in this equation represent an amalgam of the locations (μ_{tj}) of each candidate along t underlying "issue" dimensions and coefficients (γ_{st}) that linearly transform (through equation 5) individual characteristics into individual tastes for these issues.

There remains the problem of relating the expected utility in equation 6 to the observed choices in paired comparisons. This involves two difficulties. First, we only observe whether or not j was chosen, not the expected utility in equation 6. Second, some respondents profess to be indifferent between the two alternatives. The first problem leads us to a quantal choice formulation in which we assume that for individuals with the same observed characteristics Y_{is} and V_{ij}, the residual components ϵ_{ij} have a normal distribution with zero mean. Then $E(U_{ij})$ has a normal distribution as well, with mean $E[E(U_{ij})]$[9] equal to all of the terms on the right hand side of equation 6 except for ϵ_{ij}. To simplify notation, we set W_{ij} equal to $E[E(U_{ij})]$ thus:

$$E(U_{ij}) = W_{ij} + \epsilon_{ij},$$

where

$$W_{ij} = \Sigma_s Y_{is}\psi_{sj} + \psi_{0j} + \delta V_{ij}. \tag{8}$$

We can now write the probability that candidate j is preferred by person i to candidate k (j P_i k) as

$$\begin{aligned}\text{Prob}(j\ P_i\ k) &= \text{Prob}[E(U_{ij}) - E(U_{ik}) > 0] \\ &= \text{Prob}(W_{ij} - W_{ik} > \epsilon_{ik} - \epsilon_{ij}) \\ &= \Phi(W_{ij} - W_{ik}),\end{aligned}$$

where Φ is the cumulative normal distribution.

The solution to the second problem of indifference follows naturally from the preceding section where we found that preferences could be represented by a utility function and a thresholds function over all of the candidates. Hence, we assume that j P_i k only if the difference in the expected utility exceeds some threshold τ_{ik}.

$$\begin{aligned}\text{Prob}(j\ P_i\ k) &= \text{Prob}[E(U_{ij}) - E(U_{ik}) > \tau_{ik}] \\ &= \Phi(W_{ij} - W_{ik} - \tau_{ik}).\end{aligned} \tag{9}$$

Hence j is more likely to be chosen over k as the average expected utility difference between j and k ($W_{ij} - W_{ik}$) gets bigger and as the indifference threshold τ_{ik} gets smaller. We assume that the indifference threshold τ_{ik} is a linear function of uncertainty about the candidate so that it equals ξV_{ik}.[10] Then substituting from equation 8 into equation 9 letting $\pi_{ijk}(\theta)$ equal Prob(j P_i k), defining ψ_{sjk} as $\psi_{sj} - \psi_{sk}$ for all $s = 0, \ldots, S$, and letting θ represent the parameters $(\psi_{0jk}, \psi_{1jk}, \ldots, \psi_{Sjk}, \delta, \xi)$, we obtain

$$\begin{aligned}\pi_{ijk}(\theta) &= \Phi[\Sigma_s Y_{is}(\psi_{sj} - \psi_{sk}) \\ &\quad + (\psi_{0j} - \psi_{0k}) + \delta(V_{ij} - V_{ik}) \\ &\quad - \xi V_{ik}] \\ &= \Phi[\Sigma_s Y_{is}\psi_{sjk} + \psi_{0jk} \\ &\quad + \delta(V_{ij} - V_{ik}) - \xi V_{ik}]. \qquad (10)\end{aligned}$$

This is the basic equation we shall estimate in the following sections. The ψ_{sjk} provide information through equations 7 and 5 about how people's political tastes are related to their characteristics and the positions of the candidates. We shall use this information to test whether people are broadly self-interested. The parameter δ indicates whether people are risk-averse or risk takers, and ξ indicates whether there are significant indifference thresholds. These parameters reveal how limited knowledge affects people's preferences.

Equation 10 can be estimated straightforwardly once the likelihood function has been written down. Let q_{ij} equal one if j is chosen by person i, and zero otherwise, and assume a trinomial distribution for the probabilities. Then the likelihood equals, up to a constant of proportionality, the following:

$$L(\theta_{jk}) = \prod_{i=1}^{I} \pi_{ijk}(\theta)^{q_{ij}} \pi_{ikj}(\theta)^{q_{ik}} A(\theta),$$

where

$$\begin{aligned}A(\theta) &= [1 - \pi_{ijk}(\theta) \\ &\quad - \pi_{ikj}(\theta)]^{(1-q_{ij}-q_{ik})} \qquad (11)\end{aligned}$$

Operationalizing the Model

The model in equation 10 has been estimated for both the 1976 CP data and the 1983 SAQ survey. When comparing our two data sets, we found that we had some similar and some dissimilar operationalizations of the two basic theoretical constructs in our model: the uncertainties, V_{ij}, and the individual characteristics, Y_{is}. For the CP data, the uncertainty V_{ij} about each candidate was measured by a recoding of a self-report measure of knowledge with the value of one for the candidates whom respondents knew something about, two for those they knew only by name, and three for those they had never heard of. The observable characteristics of individuals Y_{is} were measured by two types of variables: (1) such socio-economic status measures as education (on the one-to-seven scale), union membership (one if either respondent or spouse belong to a union, zero otherwise); Protestant, Catholic, and Jewish dummy variables (one if a member, zero otherwise); income on a nine-point scale; ethnicity (one if white, zero if black, oriental, Spanish, or other) and (2) such attitudinal variables as liberalism-conservatism (on a five-point scale with one strongly liberal and five strongly conservative) and position on an upcoming referendum (Proposition 15) limiting nuclear power (one if definitely in favor and five if definitely opposed).

For the SAQ data, two different measures of uncertainty were used. One was a recoding of self-reported knowledge so that a value of one meant the person knew a lot and a value of four indicated the person knew nothing at all about the candidate. The second was a recoding of the willingness to place candidates on four-issue items by scoring zero if an answer was provided and one if no answer was given. When summed over the four issues, this provided a scale that could range from zero (no uncertainty) to four (very uncertain). The observable characteristics of individuals Y_{is} were measured by the respondents' positions on the seven-point items on liberalism-conservatism, support for government-guaranteed jobs, support for aid to minorities, and support for increased defense spending. As with the two attitudinal variables in the CP data, higher numbers on the scale represented more conservative positions. A total of only seven people failed to place themselves on one or more of these scales.

All of these measures have the advantage of being commonly used and exogenous to choices. Socioeconomic measures have the best claim to exogeneity, but it also seems unlikely that self-placements on the attitudinal scales are very much affected by choices among candidates for the nomination. The willingness to place and self-report measures of knowledge might be partly the result of projection from preference; but we suspect that not much of this happens at low levels of knowledge, because knowing something about a candidate is causally prior to liking or disliking the candidate. Furthermore, the rational theories of choice tested here leave no doubt about uncertainty being prior to choice.

We began our investigation of the SAQ data by estimating equation 10 for all 15 pairs of candidates,[11] using the willingness to place and self-report measures of uncertainty and the sum of these two measures. The sum yielded the highest log-likelihood for 12 of the 15 runs and the highest average log-likelihood (−118.2) across all the runs. The willingness to place measures produced an average log-likelihood of −120.3 across all the runs and did better than the self-report measures in nine cases. The log-likelihood for the self-report measures averaged −122.6.[12] If each measure is an unreliable proxy for true knowledge, we would expect to find the best fit for the sum of the two measures. At the same time, these results suggest that there is not that much difference among the three measures. We have used the sum throughout the rest of our analysis.

These measures of uncertainty can be used to determine whether preferences must be represented by the varying thresholds of interval orders and not just the constant thresholds of semiorders. Figure 1 suggests that indifference thresholds get larger as knowledge about the candidate decreases. Is this effect statistically and substantively significant, or is it simply a coincidence? Can interval-ordered preferences be explained in terms of differential knowledge about the candidates?

If semiorders are sufficient for representing preferences, then for a person i the uncertainty measures V_{ij} and V_{ik} in the indifference threshold terms ξV_{ij} and ξV_{ik} must be equal to one another for all j and k. If we call this common quantity V_i, a reasonable measure of it is the average of the V_{ij} across all of the candidates. If interval orders are necessary, then the deviations $(V_{ij} - V_i)$ of the V_{ij} from V_i must also play an important role. Hence, we respecify the indifference threshold as follows:

$$\tau_{ik} = \xi_1(V_{ik} - V_i) + \xi_2(V_i). \tag{12}$$

If ξ_1 equals zero, semiorders are sufficient for representing preferences, and if ξ_1 equals ξ_2, our original specification is correct.

Estimating the Model for the SAQ Data

We estimated both the original specification and the new one in equation 12 for all 15 paired comparisons in the SAQ data. The results show that uncertainty and self-interest are important components of choice, and the parameter estimates in all paired comparisons corroborate the claims of rational choice under uncertainty. Knowledge enters our model through both the indifference thresholds and a risk parameter. The former indicates what sort of utility function people have; the latter reveals how strongly people analyze uncertain alternatives. Our results show that an interval level model—that is, the original specification—is correct. All the estimates for ξ_2, the average level of candidate knowledge, were highly significant (at least at the .001 level) and about the same magnitude (around .09). The estimates for ξ_1, the impact of the deviations from the average level of knowledge, have larger standard errors than those for ξ_2, but 9 of them are significant at the .01 level or better and 11 meet the .05 criterion. Furthermore, a comparison of the estimates for ξ_1 and ξ_2 indicates that ξ_1 is somewhat bigger than ξ_2 in 14 cases, but likelihood ratio tests[13] suggest that we might conservatively assume that ξ_1 and ξ_2 are approximately equal. This justifies our use of the original model when we compared the various knowledge measures. Even when taken alone—but certainly when considered along with the evidence in Table 3—these results imply that preferences can be interval-ordered as well as semiordered.

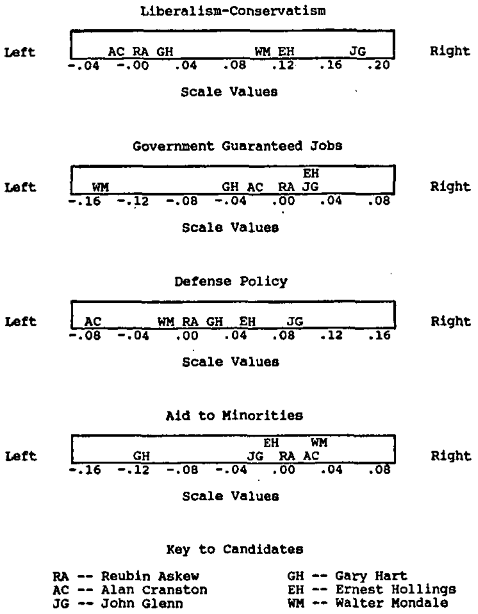
Knowledge has an additional effect on choices through the risk parameter, δ. The estimated risk parameters are all negative and highly significant (at the .001 level and beyond), and all of them lie between $-.15$ and $-.35$; that is, people tend to prefer candidates about whom they know more—they avoid risks. Risk aversion seems to play an important role in choice.[14]

Our results from the SAQ data also yielded estimates of the coefficients for issue placements, the ψ_{sjk}, in equation 10. The liberal-conservative and defense scales were highly significant in most paired comparisons, and they were more important than government-guaranteed-jobs and aid-to-minorities scales.[15] The role of issues and ideology in expressed preferences is rather complicated. As equation 7 suggests, the coefficients for the issue placements depend on the positions of each candidate on each issue (the μ_{tj} in equation 4) and the importance of the issue for the individual (the γ_{st} in equation 5). Untangling these two effects is not a trivial problem.

There are several ways to analyze these coefficients which take advantage of the constraints on the ψ_{sj} in equation 7. One approach is to assume that each self-placement scale describes a separate dimension so that we can obtain values for the μ_{tj} by regressing the ψ_{sjk} on five variables d_m (one for all but one of the candidates)—each with values of one if $m = j$, minus one if $m = k$, and zero otherwise. (The sixth candidate has a value of μ_j equal to zero, which serves as the reference point for the remaining candidates.) The γ_{st}'s are the factor loadings for each issue; that is, the γ_{st} in equation 5 indicates how much an observed issue position (s) is translated into an underlying position (t). We must, however, impose some constraints on γ_{st} to identify the model. If the issues are independent, we can standardize γ_{st} to one. Once this is done, the coefficients of each d_m represent values for μ_m. This analysis was done for all four sets of coefficients. The results are presented in Figure 3.

Except for all the placements on the aid-to-minorities scale and the placement of Reubin Askew on the remaining three scales, these figures look quite reasonable. Cranston, who was supporting the nuclear freeze, is considered the most liberal and considerably more to the left on defense than any other candidate. Mondale, who talked about maintaining the policies of the New Deal, was placed the farthest to the left on the government-guaranteed-jobs scale. Hart, who emphasized a "responsible" defense policy and a concern for the reindustrialization of the United States, was placed to the right of Cranston and Mondale on defense but between Mondale and Cranston on jobs. Glenn and Hollings, with more conserva-

Figure 3. Candidate Positions on the Issues for Respondents to the Self-administered Questionnaire



Note: Acronyms are initials of Reubin Askew, Alan Cranston, John Glenn, Gary Hart, Ernest Hollings, and Walter Mondale.

tive views, were placed on the right on the liberal-conservative, government-guaranteed-jobs, and defense scales. Moreover, Glenn, a former astronaut who expressed a concern for a strong military and often supported jobs policies, was placed to the right of Hollings on defense but to the left of Hollings on government-guaranteed jobs. This seems quite reasonable.

The placements on the aid-to-minorities scale and of Reubin Askew seem much less reasonable. We suspect that positions on the aid-to-minorities scale simply do not have very much to do with people's choices. This accounts for the relative insignificance of the coefficients for this scale, and it leads to the uninterpretable results pictured in Figure 3. The results for Askew seem more interesting. Askew was arguably the most conservative Democrat who entered the 1984 race. Yet he was placed on the left of the liberal-conservative scale and in the middle of the government-guaranteed-jobs and defense-spending scales. We suspect that people knew practically nothing about Askew and simply assumed he was centrist. If this is true, then an unknown candidate may have the advantage of starting with a useful centrist image.

Estimating the Model with the CP Data

Can we reproduce these results with our other major data set? The three candidates in the CP data for whom there was adequate information about knowledge were Carter, Jackson, and Udall. Table 4 for the 1976 CP data summarizes our results when we estimated the two different models for indifference thresholds. As before, the coefficients for separate candidate thresholds are highly significant in both the constrained and unconstrained equations, and the coefficient for an overall threshold based upon the average of 15 self-report items is also highly significant. Moreover, although the point estimate for the importance of separate candidate thresholds is larger in all three cases, it is significantly larger only in the Carter-Udall comparison.

The magnitudes of the thresholds across the two data sets can be compared if we assume that the one-to-three coding of the CP self-report measure is roughly the same as the one-to-seven coding of the SAQ combination of willingness to place and self-report measures. We think this identification makes sense, so that the values of ξ, ξ_1, and ξ_2 in Table 4 should be about 2.33 (seven divided by three) times these same values obtained for the SAQ data. The last three lines of Table 4 show that this is approximately true.

Table 5 summarizes the coefficients for the three final equations.[16] The risk aver-

Table 4. Candidate-specific Knowledge versus Average Knowledge As Explanations for Indifference Thresholds in Paired Comparisons

	Constrained Specification with Only Candidate Knowledge		Unconstrained Specification with Average Knowledge As Well		
Candidate Pairs	Log-Likelihood	Threshold for Candidate Knowledge	Log-Likelihood	Threshold for Candidate Knowledge	Threshold for Average Knowledge
Carter-Jackson	-551.22	.17 (.02)	-550.00	.24 (.05)	.18 (.02)
Carter-Udall	-537.60	.20 (.02)	-533.97	.33 (.05)	.22 (.02)
Jackson-Udall	-563.08	.22 (.02)	-561.76	.31 (.06)	.23 (.02)
Average	—	.20	—	.29	.21
Average for SAQ data	—	.10	—	.16	.09
Ratio of averages	—	2.02	—	1.84	2.32

Source: 1976 California Poll data.

Note: Standard errors are in parentheses.

Table 5. Determinants of the California Poll Preferences

	Candidate Pairs		
Explanatory Variables	Carter-Jackson	Carter-Udall	Jackson-Udall
Constant	.41 (.26)	-.44 (.25)	-.53 (.24)
SES variables			
White	.13 (.11)	.24 (.12)*	.03 (.11)
Jewish	-1.56 (.36)***	-.73 (.29)**	.39 (.28)
Protestant	-.27 (.14)*	-.01 (.14)	.16 (.14)
Catholic	-.19 (.14)	.12 (.14)	.22 (.14)
Education	-.02 (.03)	-.07 (.03)*	-.03 (.03)
Attitudinal variables			
Limit nuclear power	-.06 (.03)*	.09 (.03)*	.09 (.03)**
Conservatism	.004 (.05)	.17 (.05)**	.14 (.05)*
Risk coefficient	-.34 (.07)***	-.30 (.07)***	-.28 (.06)**
Indifference threshold	.17 (.01)***	.20 (.02)***	.22 (.02)***

Note: Positive coefficients for the SES and attitudinal variables indicate that voters with the characteristics favor the first candidate in the pair. Standard errors are in parentheses. The sample size was 569 for all three runs.

*Significant at the .05 level using a two-sided test.
**Significant at the .01 level.
***Significant at the .001 level.

sion parameters are again highly significant, although they are about equal to those in Table 4—instead of about 2.33 times bigger, as we might have expected. The coefficients on the SES and the attitudinal variables can be interpreted by summarizing the Carter, Jackson, and Udall campaigns.[17] Udall self-consciously ran his campaign as a liberal on all issues. Carter maintained a studied ambiguity on many issues, so that many voters only knew that he was an evangelical Christian and a former southern governor. Jackson was known as a very strong supporter of Israel, a believer in U.S. energy independence, and a supporter of a strong national defense.

It is not surprising, then, to find that our estimates suggest that Carter, the southerner, did somewhat better among white voters (and worse among blacks) than did Jackson and Udall, whose liberalism appealed to minorities. Members of the mostly liberal Jewish community found themselves caught between Jackson's conservatism and his strong support for Israel, but they had no difficulty in choosing Jackson over the equally conservative Carter, and they even chose Jackson over the liberal Udall. It is somewhat surprising to find that Protestants preferred Jackson to Carter, but it is not surprising to find that Catholics were wary of Carter's evangelism. Finally, it seems as if less-educated voters preferred the folksy southern manner of Jimmy Carter to the more patrician demeanor of Morris Udall.

On the attitudinal variables, Udall is seen as both an environmentalist (he picks up support among those who would limit nuclear power) and as a liberal. Jackson, as a strong supporter of nuclear and other forms of energy development, picks up support over both Udall and Carter among those opposed to limiting nuclear power. And Jackson and Carter are seen as significantly to the right of Udall. Except for the coefficient on the Protestant variable, all of these results are in accord with the positions and characteristics of Carter, Jackson, and Udall.

Conclusion

Knowledge and Individual Rationality

The preferences in our surveys were highly structured. Eighty to ninety percent of our respondents had at least interval orders; and when given a chance, those with cycles or suborders moved towards transitive preferences. At the same time, 12–25% of our respondents had interval or semiorders, and they showed no inclination to avoid intransitive indifference. Preferences are structured, but not always as weak orders. We do not think that this means that people are irrational about their interests; rather it means that people sometimes do not know enough about their interests to make a commitment one way or another.

The preferences in our surveys can be described in terms of a dimensional model of political issues that match the voter's interests with candidate's positions. Members of ethnic or religious groups, for example, voted for candidates who seemed sympathetic to their concerns. Voters chose candidates whose issue positions were congruent with their own, and these issues appear to have some underlying structure.

Knowledge about the candidates has a strong impact on choice through two routes—indifference and preference. First, all other things being equal, voters are less likely to be indifferent between a relatively well-known candidate and any other candidate than between an unknown candidate and any other candidate. Second, all other things being equal, voters prefer the candidate they know more about, avoiding less certain alternatives.

Knowledge clearly matters, but exactly what is it? Equation 10 combines two different theories of knowledge. The risk aversion term follows from the statistician's expected utility model of knowledge as a probability distribution over possible states of nature. The indifference threshold follows from the psychophysicist's concept of knowledge as the ability to discriminate between alternatives. Vincke (1980) provides an axiomatization that combines expected utility theory and indifference thresholds, but his axioms reveal little about the relationship between the ignorance created by a world in which it is hard to compare alternatives and the ignorance created by a probabilistic world.

Even if one accepts Vincke's axiomatization as a reconciliation of two different conceptions of knowledge, there is an additional problem. Our empirical tests of equation 10 rely upon still another conception of knowledge, drawn from test theory: the amount of information respondents have about candidates. Combining these approaches leads to an empirically satisfying model, but this empirical success is purchased at the cost of theoretical confusion about the nature of knowledge and its relationship to rationality.

These problems suggest that more thought must be given to developing a theory of electoral ignorance. This theory must deal with the radically unstructured nature of choices in many electoral situations, especially primary elections. Most axiom systems for expected utility assume more structure for a decision than is evident in primary election voting. Typically, consequences are denoted in cardinal terms (often money), and the probabilities of these consequences are known. This is described as a situation involving risk. If the probabilities are not known, the situation is said to involve uncertainty. We suspect that political choices are more difficult since voters must deal with ignorance: they do not even know the relevant consequences.[18] Voters must decide how to decide.

This conclusion is consistent with Kahneman and Tversky's (1974, 1979) emphasis on "framing." According to this approach, people may choose differently if mathematically "identical" lotteries are described in different ways. The empirical importance of framing demonstrates the problematic nature of the description of the situation. A political candidate is an especially complicated "lottery" with an infinite number of potential consequences and characteristics. Part of the process of learning about a candidate involves determining the relevant dimensions for evaluation. A theory of ignorance, as part of a theory of decision making, should explain how people deal with their lack of knowledge about the candidates. As people learn more about candidates, we might expect standard expected utility theories to become more and more applicable; but when voters do not know very much, we must employ a theory that explicitly deals with ignorance. This theory must, among other things, agree with our empirical results in which knowledge is used to explain indifference thresholds and risk aversion.

The Dynamics of Presidential Primaries and Collective Rationality

Many of social choice theory's very disturbing impossibility results assume that all voters can order alternatives at least weakly. If stronger assumptions can be made about individual choices, such as the existence of cardinal utility, impossibility results become harder to produce. Our empirical results, however, indicate that far from being able to strengthen these assumptions about individual choice, we must actually weaken them so that even more vexing impossibility theorems must be considered.[19] Yet we would not like to see our results lead to another spate of impossibility theorems based on assuming that individual preferences are interval orders. For one thing, this approach only considers the gross characteristics of preferences described in the first section. It ignores the detailed picture of preferences—the roles of issues and knowledge—developed in the last section. Social choice theorists have of course expended a great deal of time determining the consequences of multidimensional issue spaces. Our results suggest that as much thought should be given to the implications of differential knowledge.

The importance of knowledge can be easily seen by considering the Mondale-Hart comparison in the SAQ-I data. On the one-to-seven knowledge measure with seven indicating a great deal of knowledge, Mondale rated at 4.8 while Hart rated at only 2.3. In the paired comparison, Mondale obtained 62.3% of the vote, Hart obtained 21.6%, and 16.1% were indifferent. Using our model, we can see what happens when knowledge levels are changed. For example, we can increase each voter's knowledge of Hart to the level of his or her knowledge of Mondale. In this case, Mondale only obtains 38.3% of the vote, Hart obtains 58.6%, and only 3.1% are indifferent. Or we can decrease each voter's knowledge of Mondale by setting it equal to the knowledge about Hart. Then, Hart receives 40.1% of the vote, and Mondale 24.1%, and 35.8% are indifferent. As expected, as knowledge increases, the fraction of indifferent voters goes from 35.8% to 16.1% and finally to 3.1%. What is impressive is the magnitude of these numbers. Equally impressive and politically more significant is how Mondale's vote changes from case to case. Indeed, Mondale only seems to enjoy a plurality in the initial situation because people know so much about him. Hart's rise to prominence after the Iowa caucuses in 1984, then, must have had a great deal to do with increased knowledge about him.

The social choices offered by presidential primaries, it seems, are fundamentally shaped by how information is generated by the media and processed by individuals. Yet most social choice theorists[20] have neglected these influences while developing an extensive literature on the effects of different distributions of preferences and different decision-making rules. The "new institutionalism" (Schwartz 1986, pt. 4; Shepsle 1984) has begun the task of adding institutional realism to social choice theory, but this approach has tended to focus on legislatures where such multistage decision processes as committee systems are the major institutional feature. We believe that equal attention should be paid to how the media's presentation of information and the voters' limited information-processing capabilities affect social choice.

Appendix A: Description of Data Sets and Questions

The California Poll was conducted in respondents' homes by the Field Research Corporation, which has been polling in California for over 30 years. The California Poll is considered one of the most carefully conducted statewide surveys in the country. The SAQ was completed by respondents on computer terminals attached to the Berkeley Computer Assisted Telephone Interviewing (CATI) system. Respondents to the SAQ were told that the researchers were interested in their political attitudes and opinions, and the SAQ ranged over a variety of topics including political tolerance, political opinions, and voting preferences in the following year's presidential primaries.

A subset of 611 Democrats and Independents in the CP were asked a full set of 10 paired comparisons for the five Democratic presidential aspirants—Jimmy Carter, Henry Jackson, Jerry Brown, Hubert Humphrey, and Morris Udall. The exact question was,

> Here is a list (Card C) of some people who are candidates or who have been mentioned as possible democratic presidential nominees in this year's election. On this card (Card D) some of these men are matched against each other in sort of a paired comparison test for the upcoming presidential race. For each pairing please tell me which candidate you prefer, assuming the choice was just between those two men. Let's take the first pair—Carter against Jackson—who[m] do you prefer?

In addition, a random half-sample of 307 of these 611 Democrats were also asked, somewhat later in the interview, about Gerald Ford versus Edward Kennedy, Hubert Humphrey, Jimmy Carter, Henry Jackson, Jerry Brown, Morris Udall, and some other candidates:

> Here is a card (Card L) which lists a number of possible pairings in this year's presidential race. . . . Let's take the first one—where Edward Kennedy might be facing Gerald Ford. If you had to vote today for U.S. President, who would you vote for—Kennedy or Ford?

These questions provide 307 sets of 15 paired comparisons involving Carter, Jackson, Brown, Humphrey, Udall, and Ford. Indifference was not mentioned as a possible response, but respondents could indicate no preference.

Towards the end of the SAQ (after about a half hour of other political questions), respondents saw the following on their computer screen:

> We are now going to ask you a series of PAIRED COMPARISON questions concerning several possible Democratic candidates. After you answer all of the paired comparisons YOU WILL BE SHOWN ALL OF YOUR PREVIOUS ANSWERS AND YOU WILL BE GIVEN A CHANCE TO CHANGE AS MANY OF YOUR ANSWERS AS YOU WISH (up to a maximum of 20 changes).
>
> If just [candidate one] and [candidate two] were running for the Democratic nomination so that the choice was just between these two men, who would you prefer?
>
> 1. [Candidate One]
> 2. [Candidate Two]
> 3. I'm really not sure.

Each respondent was presented with 15 paired comparisons involving the six Democratic candidates Reubin Askew, Alan Cranston, Gary Hart, Ernest Hollings, Jesse Jackson, and Walter Mondale. The CATI-SAQ system substituted these names in the brackets and recorded each answer. After completing the 15 comparisons, each respondent was presented with a summary of all 15 choices and given the opportunity to change his or her answers. Only one respondent made more than six changes, and 135 made no changes.

Notes

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1. Thomas Schwartz (1986) notes in passing that "it is generally and pretty much uncritically assumed that these conditions [for rational choice] hold for individual choice-processes" (p. 17).

2. In measurement theory (Krantz et al. 1971; Scott and Suppes 1958; Suppes and Zinnes 1963), one basic task is to find an homomorphism between an empirical relational system (e.g., the responses to paired comparison questions that we use here) and a numerical relational system (e.g., the real numbers and their arithmetic properties). Another basic task is to obtain strong measures that utilize a great deal of the structure of the real number system (e.g., not just the ordinal properties of numbers but their additive properties as well).

3. Dummett provides an especially pungent description of this possibility (1984, 34–35). The same kind of argument provides a justification for the independence axioms of objective and subjective expected utility.

4. An alternative way of developing rational choice theories focuses on choices from choice sets and their subsets. For a variety of historical reasons (nicely surveyed in Sen 1973) economists have become enamored with the choice function formulation, but we think Sen is correct in arguing that there is really no logical or methodological reason to prefer one approach over the other. Because we have available several sets of paired comparisons, it makes sense to present axioms based on binary preferences.

5. Thus a utility function that assigns the numbers 10 to j, 7 to k, 7 to m, and 4 to n implies that $j\ P\ k$, $j\ P\ m$, $j\ P\ n$, $k\ P\ n$, $m\ P\ n$, not $k\ P\ m$ and not $m\ P\ n$.

6. Luce (1956) was the first to provide a set of axioms for a semiorder; Fishburn (1970a) provides a survey of the literature on transitive indifference, and Fishburn (1970b, 1973) and Roberts (1970) provide a thorough discussion of representation theorems for interval and semiorders.

7. "Consistency of interests" could be called a form of "self-interest," but we reserve this latter phrase for actions that are in accord with one's own needs and wants. Consistent interests might satisfy this requirement, but could just as well be at odds with it.

8. In the SAQ survey we did ask respondents to place the candidates on various issues, but more than half the respondents did not place Askew, Glenn, Hart, and Hollings on the straightforward "government-guaranteed-jobs" question. Moreover, we suspect that the placements of these candidates were greatly affected by projection (Brady and Sniderman 1985).

9. Note that the outermost expectation is taken over all individuals with characteristics given by the Y_{is} and V_{ij} values while the inner expectation is over the possible states of the world as perceived by an individual of type i. The inner expectation is an individual expected utility, whereas the outer expectation is an average random utility.

10. We have assumed that V_{ik} has a meaningful zero point so that the intercept in this linear transformation can be set to zero. In the estimations reported below, we have found that this is a reasonable assumption.

11. We used the initial paired comparisons from the SAQ data. The final paired comparisons are not much different for the purposes of estimating this model.

12. We wrote a program in GAUSS to maximize the likelihood in equation 11 by starting with a BHHH iteration (Berndt et al. 1974), continuing with BFGS iterations (Broyden 1967) until convergence was achieved (every gradient less than .00001), and ending with a BHHH iteration to compute the variance-covariance matrix of the parameters.

13. A likelihood ratio test of the equality of ξ_1 and ξ_2 can be based on the likelihood for the model when ξ_1 equals ξ_2 and the likelihood when these parameters are left unconstrained. This test has a chi-squared distribution with one degree of freedom. The .10 value for a chi-squared is 2.706, and only three pairs (Glenn-Askew, Hollings-Cranston, and Glenn-Hollings) attain this size. Of course, this is not a joint test across all 15 equations, and the average values for ξ_1 and ξ_2 suggest that ξ_1 is about 73% bigger than ξ_2.

14. Our results for the indifference thresholds and risk aversion coefficients might be spurious if our measures of knowledge are nothing but projection from utilities. However, we have found that we still get an impressive indifference threshold effect when we use a clearly exogenous variable such as education instead of average knowlede. Moreover, we get the same results with the somewhat more objective willingness-to-place measures of knowledge as with self-report measures. Finally, when we use an econometric technique to test for projection, we cannot find very much. A full description of these arguments and our evidence against spuriousness is presented in a working paper available from the authors.

15. The coefficients for the liberal-conservative and defense scales are larger than their standard errors in six cases, while the coefficients for the government-guaranteed-jobs and aid-to-minorities self-placements are larger in three and four cases, respectively. Furthermore, the range of the coefficients is largest for the liberal-conservative scale (3.57), almost as large for the defense scale (3.40), and substantially less for the government-guaranteed-jobs and aid-to-minorities scales (2.93 and 2.68, respectively).

16. We also included union membership and income in our earlier runs, but none of the coefficients were larger than their standard errors.

17. We have used Jules Witcover (1977) as our source of information on the 1976 campaign.

18. A proponent of subjective expected utility (SEU) theory might argue that ignorance poses no real problem for SEU because a voter could still list all possible consequences and assume an "ignorance" prior. This approach founders on the limitations of human information-processing capacity that make listing all possible consequences unlikely if not impossible, and the well-known ambiguities involved in defining ignorance priors.

19. See, for example, Theorem 4.22 in Schwartz 1986, which demonstrates the impossibility of assuming a set of voters with semiordered preferences and a decision rule that satisfies the strong Pareto condition and collective acyclicity. Social choice theorists have proven many theorems like

this one.

20. There are some important exceptions, such as Shepsle 1972, Calvert 1984, and the very important series of papers by Richard McKelvey and Peter Ordeshook, for example, McKelvey and Ordeshook 1985.

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Henry E. Brady is Associate Professor of Political Science, University of Chicago, IL 60637.

Stephen Ansolabehere is a Ph.D. candidate, Department of Government, Harvard University, Cambridge, MA 02138.

BLACKS AND HISPANICS IN URBAN POLITICS

KENNETH R. MLADENKA
Texas A&M University

Are local political institutions responsive to the demands of racial minorities for a more equitable share of scarce public resources? Does an enhanced political presence improve black and Hispanic prospects in the struggle for public jobs? Based upon data for 1200 cities, my analysis reveals that while minority council members make a substantial contribution to employment success, minority mayors do not. For blacks, an interactive effect was also discovered for the relationship between the level of political representation, governmental structure, and employment outcomes. This research reveals that the political process does work. However, its effectiveness for minorities is mediated by a complex of factors that includes an interaction between political power, racial polarization, and the nature of local political institutions.

After decades of demand and protest, are racial minorities any closer to achieving an equitable share of public sector resources? After numerous legal victories, legislative successes, and affirmative action initiatives, are blacks and Hispanics reaping tangible material rewards as just recompense for their labors? Does the political process work? If racial minorities elect mayors and council members, can they expect an increased flow of benefits from local public office? Does a heavy and consistent investment in the political system pay substantial dividends?

Given the crucial importance of the questions, surprisingly little research addresses the issue. One group of researchers does find that the election of blacks to office produces increased benefits for the black community (Campbell and Feagin 1977; Cole 1976; Keech 1968; Levine 1974). However, Karnig and Welch (1980) examine municipal expenditure patterns and discover that while black mayors had a relatively small effect upon policy, black council members did not. In addition, the literature on the distribution of urban public services reveals that resources are distributed in accordance with bureaucratic decision rules and technical-rational criteria rather than in response to political factors (Lineberry 1977; Mladenka 1980).

However, several recent examinations of the minority share of city jobs make a strong claim for the efficacy of black political power. It is reasonable to assume that blacks would focus their political energies on public sector jobs since according to Browning, Marshall, and Tabb (1984, 169), "governmental employment has long been an important goal and point of early access for excluded groups in American society." These authors further observe that "discrimination in employment was one of the prime targets of black protests in the early Sixties. Several federal programs included in their stated goals the elimination of discrimination in public employment or the employment of the poor in general or of minorities in particular. Among these were the poverty program (1964), Model Cities (1967), and

Equal Employment Opportunity (1972). At the local level significant minority political activity focused on changing city policy regarding government employment" (p. 169).

A variety of studies provide varying degrees of support for the argument that vigorous efforts to increase the number of black elected officials in municipal government have had a demonstrable effect upon the minority share of city jobs. Dye and Renick (1981) find that although black representation on city council exerts no influence upon the black proportion of total city employment, it is the *most* significant determinant of administrative, professional, and protective service (policemen and firemen) positions. They conclude that "employment in top city jobs appears to be a function of political power, as it is reflected in city council representation. Representation on city council is more important in gaining city employment in administrative, professional and protective positions than any other single factor. . . . We believe that black representation on city councils is a crucial link in improving black employment opportunities in city government" (pp. 484–85).

Eisinger (1982) has also discovered that the number of black elected officials is unimportant in accounting for the variation in the minority share of *total* city jobs. The important variable here is the size of the black population. For decisionmaking and professional positions, however, Eisinger finds that the presence of a black mayor has a significant impact upon increasing the level of black employment. Although the black population was again the single, most important determinant of minority job success, a black chief executive did make a difference. Eisinger concludes that "the presence of a black mayor has a modest incremental effect on levels of black employment. . . . enabling us ultimately to conclude that a small but discernible portion of black employment is a product of black political authority" (p. 391).

In yet another study, Stein (1986, 702) analyzes the variation in the minority share of the workforce for the total jobs variable. Although percentage minority population was the most significant factor, Stein also finds that "political factors . . . play significant roles." Specifically, the presence of a minority mayor and a mayor-council form of government were important variables in accounting for the variation in the employment of racial minorities.

Browning, Marshall, and Tabb (1984) study 10 California cities and find that the level of black representation on council was much more important than either demographic or bureaucratic factors in accounting for increases in black employment. They observe that they "find little support for the hypothesis that socioeconomic or demographic variables are more important than political variables in explaining policy; variations in city government responsiveness are not well explained by the proportion of minorities in the population. Cities with relatively small proportions of minorities were sometimes more responsive than cities with larger minority populations. Other factors more directly political in nature . . . yield a much stronger explanation" (p. 199–200).

In a related vein, Saltzstein (1986) analyzed female employment patterns in a number of cities. Her results also provide support for the argument that minority political power makes a substantial difference. Specifically, she found that the presence of a female mayor had a significant impact on increasing the level of female employment in nonclerical and professional-administrative jobs.

The conclusion that the political process works for blacks is an important finding. It is reassuring to learn that if racial minorities travel the electoral route, they can translate ballot success into tangible, material rewards. Black mayors mean more jobs for the black community. These jobs, in turn, will increase minority influence upon public policy. Eisinger (1982, 391) goes so far as to say that job progress will "contribute to the formation of a black bureaucratic middle class." I conclude from this line of research that the election of minority officials, particularly mayors, will improve the chances of their constituents in the competitive struggle for scarce public resources. However, some lingering doubts remain.

First, the sample sizes in the studies described are generally small. Dye and Renick analyze only 42 cities, Eisinger includes 43 in his study, and Browning, Marshall, and Tabb study 10 cities. Stein examines employment patterns in 134 cities. Second, only Eisinger (1982) analyzes the impact of both black mayors and council members upon employment patterns. Dye and Renick limit their study to an examination of minority council while Stein analyzes only minority executives. In addition, the various studies examine different dependent variables. Only Dye and Renick analyze the full range of employment categories: total jobs, official-administrator positions, professional positions, and protective service workers. Eisinger does not include protective service positions in his analysis, while Stein limits her study to the total jobs variable. Further, the small sample sizes preclude the possibility of more detailed analysis. For example, the significance of region in accounting for variation in minority employment patterns cannot be ascertained. In addition, the various studies differ with respect to the relative importance of black political power. Browning, Marshall, and Tabb (1984) conclude that minority representation provides the dominant explanation of black employment levels, while Eisinger (1982, 391) finds that the "presence of a black mayor has a modest incremental effect." Further, very little research has examined the relationship between Hispanic political representation and employment outcomes. Welch, Karnig, and Eribes (1983) find, however, that neither Hispanic mayors nor council members were able to influence employment patterns. Finally, with only one exception (Stein 1986), past research has not considered the interaction among such important variables as local political institutions and the effectiveness of minority political power.

Methodology

The Equal Employment Opportunity Act of 1972 requires all cities with 99 or more employees to submit a report each year to the U.S. Equal Employment Opportunity Commission. The report, known as EEO-4, provides information on the race and sex of employees in eight different job categories. Since the law prohibits the release of employment data for individual cities, it is necessary to obtain the information directly from the city. A request was sent to all cities over 10 thousand in population for copies of their EEO-4 report for 1984. Two follow-up mailings generated a response from 1,350 of the 1,913 cities. Eventually, 1,224 cities were coded for analysis for a response rate of 64%. The cities in the study are highly representative of national employment patterns. Aggregate figures released by EEOC for 1984 reveal that 20.9% of all municipal jobs were held by blacks, while 10.7% of administrative, 14.6% of professional, and 12.6% of protective service positions were filled by blacks. In the sample of cities, 20.1% of all jobs were held by blacks while the comparable figures for administrative, professional, and protective service positions are 9.6%, 12.6%, and 12.1% respectively. In addition, 75% of all black mayor cities (over 15 thousand in population) are included in the study.[1]

The eight job categories are officials-administrators, professionals, protective service workers, office and clerical, paraprofessionals, skilled craftsmen, service-maintenance workers, and technicians.[2] In the analysis that follows we will concentrate upon the black and Hispanic share of total jobs, officials-administrators, professionals, and protective service workers. These three job categories are particularly significant for several reasons. First, these positions pay considerably more than clerical and service-maintenance jobs. Second, blacks and Hispanics in official-administrator (department heads, directors) and professional positions have great potential to influence the recruitment and advancement of minority employees. In addition, the presence of minorities in key decision-making positions significantly increases minority influence in vital areas of city government such as spending, taxing, zoning, regulation, housing, and public services. Finally, blacks and Hispanics in protective service positions (police officers) can have major substantive as well as symbolic value with respect to the treatment of minorities in police-citizen encounters.

Independent Variables

Several different sets of independent variables were employed in the analysis. They include the following:

Demographic
total population
percentage black population
percentage Hispanic population
education level
median income
median housing value

Political Power
presence of a black or Hispanic mayor
percentage of city council that is black or Hispanic
black or Hispanic council representative ratio

Political Structure
reform index
percentage of council elected from wards and at-large

Region
Northeast, Midwest, South, West

Composition of Municipal Workforce
percentage of all city jobs accounted for by service-maintenance and clerical positions

Percentage of Workforce That Is Black

Residency Requirements
type of residency requirements in effect as a condition of employment

Previous research reveals that the size of the minority population is a major determinant of the variation in employment levels. Specifically, the percentage black and Hispanic variables were most significant in accounting for the minority share of total jobs and less important for official-administrator, professional, and protective service positions. The explanation advanced is that market factors will determine the level of black employment for less important jobs while political influence will assume increasing significance for key positions. According to this assumption, whites are not interested in many city jobs. Consequently, the size of the black and Hispanic population will determine the level of minority employment for these positions. Decision making, professional, and police positions, however, will be targets of minority political activity, at least for blacks. Therefore, we should expect that the percentage black population variable will be significant for total employment but that political factors will become important for key city jobs. The research evidence suggests that the level of political representation will be less significant for Hispanics (Welch, Karnig, Eribes 1983).

The environment in large cities with wealthier, better educated populations should also provide more employment opportunities for blacks and Hispanics. More cosmopolitan cities are apt to be somewhat more tolerant of racial minorities (Dye and Renick 1981; Stein 1986). It is hypothesized, therefore, that large cities with higher family incomes, housing values, and educational levels will hire more minorities in all employment categories. Because of the multicollinearity among these variables and for reasons of parsimony, z-scores were calculated to create a housing-income-education index. This index will be employed in each of the regression models.

Past research suggests that black elected officials will exert an impact upon employment levels, particularly with respect to key decisionmaking and professional positions. In particular, black mayors will reward their constituents with more jobs (Eisinger 1980, 1982; Stein 1986). Nelson and Meranto (1977) find that Carl Stokes in Cleveland and Richard Hatcher in Gary were able to improve black employment in city government during their terms in office. Saltzstein (1983) has discovered that the mayor's choice of personnel administrators was crucial to effective affirmative action programs.

Black representation on city council should also influence employment levels. Dye and Renick (1981) have discovered, in fact, that black council representation was the most significant determinant of the minority share of decisionmaking and professional positions. However, Eisinger (1982, 388) reports no such effect. Instead, he finds "that seats on the city council offer black representatives little opportunity for influence over personnel policies" and that this "suggests the degree to which employment policy is an executive rather than a legislative function." In a related vein, Karnig and Welch (1980) find that black council members have no effect upon policy expenditures. On the basis of the available evidence it is reasonable to expect that minority mayors, who are apparently well positioned to influence the hiring process, will exert a larger impact upon black employment levels than minority council members. There is less reason to expect that the level of Hispanic political representation will influence employment policy. Welch, Karnig, and Eribes (1983) find that Hispanic mayors and council members have no effect upon employment patterns.

In measuring the level of minority political representation I had to decide which year to employ. Once elected to office, how long will it take before minority public officials are in a position to influence employment policy significantly? The problem is complicated by the fact that in some cities minority representation in office is not continuous. Periods of a minority presence in government are interrupted by years in which no blacks or Hispanics hold public office. Since I could find no compelling reason to prefer one year or method over another, I decided to measure the level of minority political representation during three separate periods: 1979, 1982, and 1984. The analysis revealed similar results for each year. Therefore, I will report findings for 1984 only.

A final political power variable is the council representation ratio. This measure was calculated by dividing the percentage black or Hispanic on council by the percentage black or Hispanic in the city population. The intent is to assess the extent to which councils where the minority share of the seats approaches the minority share of the population are able to influence employment patterns.

The political structure variables include election type, form of government, and percentage of council members elected from wards. I then combined these to create a reformism index. Municipal elections were coded as either partisan or nonpartisan. Partisan elections typically imply that the electoral process is contested by opposing political parties. Nominations are controlled by the parties, issues achieve greater significance, and candidates are identified on the ballot according to party designation. Nonpartisan elections tend to be dominated by personalities rather than issues. No political organization exists to provide support for public officials attempting to adopt innovative, redistributive, or controversial policies. In addition, voter turnout is lower in nonpartisan systems than in partisan ones, particularly among lower-income voters (Alford and Lee 1968; Hamilton 1971). The absence of political parties depresses turnout because the activity engaged in by parties stimulates interest, dramatizes issues, and mobilizes the voters. In addition, the lack of candidate designation on the ballot according to party affiliation contributes to voter frustration on the part of low-income citizens. It is probable, therefore, that the active electorate in nonpartisan cities will be dominated by conservative voters. Consequently, redistributive policies are unlikely to discover a sympathetic reception in such an environment.

The literature also suggests that form of government will influence public policy. Generally, mayor-council cities are thought to be more responsive to racial minorities (Lineberry and Fowler 1967) than council-manager cities. The latter type of government, with its professional city manager, is thought to emphasize middle-class values of efficiency and effectiveness and is seen as less sensitive to the demands of minority groups. Further, Stein (1986) finds that minority employment levels were somewhat higher in mayor-council cities. It should be noted, however, that other researchers have discovered that form of government exerts little impact upon municipal policy (Morgan and Pelissero 1980).

The representational system may also make a difference. Cities select council members by wards, at-large, or by a combination of the two. Generally, it is thought that ward systems are representative of neighborhood interests while at-large arrangements work to the advantage of business and upper-class interests. The empirical evidence tends to support the conclusion that minority representation on city council increases in a ward system (Engstrom and McDonald 1981; Hamilton 1978; Robinson and Dye 1978; Vedlitz and Johnson 1982). However, others have found no such relationship (Karnig 1979; MacManus 1978).

Given the nature of the evidence, we can expect that cities with mayor-council government, partisan elections, and ward systems will be somewhat more likely to hire minorities than cities with council-manager forms, nonpartisan elections, and at-large arrangements. In addition, the interactive effects among the variables may well be significant. Stein (1986) finds that minority employment levels are higher in mayor-council cities with a minority mayor than in council-manager cities with a white manager-mayor.

It should be noted, however, that form of government, partisanship, and election type all measure a more general dimension of reformism. Following the example of Lyons (1978, 123) I summed the number of reformed structures in each city (city manager, nonpartisan elections, percentage of council at-large) and assigned a reform score on that basis. These scores ranged from 0 to 3. I classified a city as reformed if its score exceeded 1.5 and unreformed if it had a score of 1.5 or less. For example, I considered a city reformed if it had a manager or nonpartisan elections and if at least 50% of the city council was elected from at-large positions.

Each of the 1,224 cities was also coded according to its regional location: Northeast, Midwest, South, and West. Although the sample sizes in most previous studies have not been large enough to control for the influence of region upon employment patterns, Saltzstein (1986) reports a negative effect for the Northeast for female jobs, while Stein (1986) has discovered higher minority employment rates in Sunbelt cities. In addition, Welch, Karnig, and Eribes (1983) analyze southwestern cities and find that individual state locations are significant in accounting for variations in Hispanic employment levels.

Another variable included in the analysis is percentage of the total workforce accounted for by service-maintenance and clerical positions. This factor was thought to be important because some municipal workforces are composed of a higher percentage of menial jobs than others. It is also the case that blacks tend to disproportionately occupy these positions. Therefore, some cities may employ more blacks than others because a larger share of the public sector workforce is accounted for by service-maintenance and clerical jobs. In fact, Saltzstein (1986) finds that the proportion of total employment that is clerical is a significant predictor of the variation in female employment levels.

For Hispanics, I also consider percentage of the workforce that is black. Hall and Saltzstein (1976) find a negative relationship between proportion of Hispanics and blacks in the municipal workforce. Welch, Karnig, and Eribes (1983) have discovered that Hispanic employment declined in cities with a large percentage of blacks on the workforce.

The effects of residency requirements upon the minority share of city jobs were also analyzed. Little is known about the impact of such ordinances on employment outcomes. However, on the basis of knowledge of the exclusionary effects of zoning regulations, I speculate that such ordinances are significant. In fact, Stein (1986, 702) finds that "the presence of a residency requirement . . . has a favorable effect on minority hiring." Data on residency requirements were obtained from the personnel directors or city clerks in all 1,224 cities. Initially, it was anticipated that they would be relatively simple in nature. Instead, U.S. cities employ a bewildering variety of residency requirements as conditions of obtaining and holding jobs with city government. They include the following:

1. All or some employees have to move into the city within a specified period of time *after* they are hired.
2. Some employees, such as policemen and firemen, have to live within a certain number of miles or minutes of travel time to the city.
3. All employees have to be residents of the city a certain number of months or years before they are eligible to apply for a job.
4. Residents are given extra points on civil service exams.
5. Residents are hired before nonresidents.
6. The city has no residency requirement.
7. Certain employees, such as policemen and firemen, have to be residents of the city a number of months or years before they are eligible to apply for a job.
8. Certain employees, such as policemen and firemen, have to be residents of the *county* a number of months or years before they are eligible to apply for a job.
9. The city has multiple residency requirements consisting of various combinations of the conditions specified above.

The interpretation of these requirements poses some difficulties. For example, an ordinance that requires a new employee to move into the city within a few months may be just as exclusionary as one that requires applicants to be resi-

Table 1. Multiple Regression of Black Employment Categories, All Cities

Variable	Total Employment	Officials-Administrators	Professionals	Protective Service Workers
Total population	.02	.04	.11**	−.01
Percentage black population	.80**	.54**	.62**	.83**
Black mayor	.00	.11**	.05**	.06
Percentage black council	.05*	.21**	.14**	.01
Percentage workforce clerical and service	.04**	.01	.03	.02
Reform index	−.02	.01	.00	−.02
Income-housing-education index	.10**	.01	.04	.06*
Black representation ratio	.00	−.06**	−.03	−.01
R^2	.85	.56	.58	.72

*Significant at .05 level.
**Significant at .01 level.

dents of the jurisdiction. A black resident of the inner city would lack the resources to move to a suburban location even if he or she happened to be qualified for the job on other grounds. Therefore, various combinations of residency requirements were coded for analysis and employed as dummy variables. A brief description of each follows.

Residency 1 0 = no residency requirement,
1 = preemployment requirement

Residency 2 0 = no residency requirement,
1 = pre- and postemployment requirement

Residency 3 0 = no residency requirement and postemployment requirement,
1 = preemployment requirement

Residency 4 0 = no requirement,
1 = preemployment requirement and unspecified resident preference

Residency 5 0 = no residency requirement and post employment requirement,
1 = preemployment requirement and unspecified resident preference.

Black Employment

An examination of Table 1 provides only limited support for the argument that black success at the ballot box translates into tangible rewards in the form of city jobs. The black mayor and black representation ratio variables exhibit a consistently weak relationship with employment outcomes in all four job categories. The black mayor coefficients are only .00 for total employment, .11 for officials-administrators, .05 for professionals, and .06 for protective service positions. However, the black council variable makes a greater difference. The council coefficients are .21 for officials-administrators and .14 for professional positions.

The political structure of city government is unrelated to how well blacks fare in the struggle for municipal jobs. An examination of the table reveals that none of the coefficients for the reform index rises above .02. It was also expected that percentage of the municipal workforce composed of clerical and service-maintenance jobs would prove to be a significant determinant of the variation in black employment. However, the evidence demonstrates that not to be the case. The same is true for the various categories of residency requirements imposed as a condition of city employment. The regression models reveal them to be consistently without effect.

Similarly, the total population variable and the income-housing-education index make little difference. The largest coefficient for the former variable is .11 while the strongest for the latter is .10. Instead, it is the size of the black population that is of overwhelming importance in each of the regression models. The coefficients for percentage black are .80 for total employment, .54 for officials-administrators, .62 for professionals, and .83 for protective service positions. What we can conclude is that it doesn't appear to make much difference whether a city is large or small, rich or poor. Further, the primary determinant of the variation in black employment levels apparently has little to do with whether the city has a black or white mayor, whether the governmental structure is reformed or unreformed, or whether the black share of seats on council approaches the black proportion of the population. Instead, black employment is a function of the size of the black population. Cities with a large black community hire more blacks than cities with a smaller black population. This powerful relationship holds for all four employment categories.

However, the level of black representation does make a difference. Although black control of the mayor's office was not an important factor, the black share of council seats did achieve modest significance. Even here, however, the influence of black representation upon employment levels is not uniform across all employment categories. Only for official-administrator and professional positions do minority council members exert an impact.

Another perspective on the relationship between the demographic, political, and employment variables is provided by eliminating from the analysis those cities with less than 10% black population. Generally, past research has excluded cities with a small black community based upon the assumption that a minimum threshold is necessary before black expectations with respect to electoral success and political power can be realized. When this is accomplished and the data are reexamined, the size of the black population again provides the dominant explanation of the variation in black job success in all four employment categories. In addition, the conclusion that the presence of a black mayor has no impact upon minority employment levels is reaffirmed. Further, the importance of the black council variable for professional positions declines to the point where it is no longer statistically significant. On the other hand, the regression coefficient for black council for officials-administrators increases from .21 for all cities to .26 for cities with large minority populations.

In general, when the analysis is limited to cities with larger black populations, the results support the findings reported for the full sample. The size of the black population continues to provide the dominant explanation of minority employment success in the public sector. This variable alone tends to dwarf in importance the political representation factors. In addition, no evidence was uncovered to challenge the conclusion that the presence of a black mayor makes little difference. However, there are two exceptions to the observation that the results for the two samples are essentially identical. First, the black council variable is no longer a significant factor in accounting for variation in professional positions. Second, the level of black representation on council increases in importance for official-administrator positions.

The finding that black political power is not particularly significant in terms of its impact upon minority employment levels is surprising. Could it be that the full sample of cities conceals considerable varia-

Table 2. Multiple Regression of Black Employment Categories, Northeast and West

	Total Employment		Officials-Administrators		Professionals		Protective Service Workers	
Variable	Northeast	West	Northeast	West	Northeast	West	Northeast	West
Total population	.06	.06	.00	.09*	.12	−.06	−.02	.02
Percentage black population	.77**	.64**	.63**	.46**	.58**	.55**	.76**	.48**
Black mayor	.01	.02	−.07	.07	−.02	−.06	.20**	.06
Percentage black council	.18*	.35**	.36**	.44**	.29*	.45**	−.03	.44**
Percentage workforce clerical and service	.04	.09**	.02	.03	−.07	.11**	−.07	.08**
Reform index	.00	.01	.04	−.01	−.10	−.02	−.04	−.01
Income-housing-education index	.14**	.09**	.06	−.04	.09	.05	.05	.05
Black representation ratio	−.02	−.09**	−.10	−.14**	−.11	−.10*	.04	−.11**
R^2	.87	.88	.69	.78	.67	.73	.79	.77

*Significant at .05 level.
**Significant at .01 level.

tion on a regional basis with respect to the effectiveness of minority political activity? Since the sample is so large, separate analyses make it possible to examine hundreds of cities in each region. When this is accomplished, some significant findings emerge. An examination of Table 2 reveals that the level of black political representation is of major importance in accounting for black job success in the West and is consistently significant in the Northeast. However, the black power variable only occasionally achieves significance in the Midwest and South.

Minority representation clearly makes a difference in northeastern cities. Although the size of the black population remains the dominant variable for all job categories, the percentage of the council that is black becomes the next-most-significant variable in the model. The regression coefficients for black council are .18 for total jobs, .36 for officials-administrators, and .29 for professional positions. Further, the presence of a black mayor becomes a significant factor in the regression model for protective service positions. The coefficient for the black mayor variable is .20. For northeastern cities, the variation in the level of minority employment is clearly a function of two factors: black population and the level of black political representation.

But while political power makes a substantial difference in determining who gets what in the Northeast, it doesn't count for much in the Midwest. The strongest relationship is noted for the black council variable and professional positions (.26). Neither black mayors nor black council members are capable of producing favorable and consistent employment outcomes in midwestern cities. Black political power also fails to make much of a dent in employment patterns in southern cities. The strongest relationships are observed for official-administrator positions where the regression coefficients are .27 for black mayor and .15 for the black council variable. Although demography rather than politics determines employment outcomes in southern cities, when politics does count, it is black mayors rather than black council members who are able to achieve favorable results for their constituents.

In the West the effectiveness of black political power is most dramatically realized. In fact, politics rivals the size of the black population as the key factor in accounting for the variation in minority job success. In western cities, the black share of council seats is a crucial variable in determining why blacks fare well in some municipalities and abysmally in others. For example, the regression coefficients for percentage black council are .35 for total employment, .44 for officials-administrators, .45 for professionals, and .44 for protective service positions. Again, however, the black mayor variable is not significant. The largest mayoral coefficient is only .07.

The separate regional analyses provide some striking evidence with respect to the effectiveness of the political process. What we can conclude is that minority investment in the political process returns substantial dividends in some parts of the country but not in others. In the West, black representation on council provides more than a simple boost to black employment chances. It is a decisive factor. In northeastern cities black political clout is an important but not dominant factor in the scramble for city jobs. In the South and Midwest political representation has some slight effect, but that impact is limited at best. In addition, the earlier finding that black mayors fail to contribute substantially to their constituents' employment success is reaffirmed. However, there are two notable exceptions to this pattern. In the North, the presence of a black mayor exerts a significant influence upon the level of minority employment for protective service positions. Similarly, black mayors make a difference for official-administrator positions in southern cities.

The examination of cities according to regional divisions strongly suggests that interpretations based solely upon an evaluation of findings derived from a national sample may well be misleading. The discovery in the full sample of 1,224 cities of a modest relationship between the level of black representation on council and job success does not hold when regional biases are included in the model. Instead, politics exerts a powerful and consistent effect in the West, substantial impact in the Northeast, and only limited influence in the South and Midwest.

Region and Political Structure

To conclude that the effectiveness of black political power varies considerably on the basis of region is not a particularly satisfying explanation. Do regional distinctions simply mask certain attributes that clusters of cities share in common? For example, do western cities exhibit a similar pattern of behavior because cities in that region have tended to adopt reformed rather than unreformed governmental structures? The regional distinctions we have uncovered might decline in significance if controls were instituted for additional variables.

The nature of local political institutions is a likely candidate for exploration in this area. The literature is replete with findings strongly suggesting that unreformed governments are more open and responsive to the diverse racial and socioeconomic groups in the city. It will be recalled that an examination of the various regression models revealed that the reform index variable was consistently without effect. Unreformed cities do not provide the black community with a larger share of public jobs. However, an interactive effect may be present. Although unreformed institutions and structures may not directly influence the level

Table 3. Multiple Regression of Black Employment Categories, Unreformed Cities with 10% or More Black

Variable	Total Jobs	Officials-Administrators	Professionals	Protective Service
Total population	.12	-.11	.35*	.04
Percentage black population	.45**	.19	.32*	.60**
Black mayor	-.04	.07	-.01	-.07
Percentage black council	.39*	.55*	.58**	.38
Percentage workforce clerical and service	.09	-.08	-.01	-.08
Income-housing-education index	.02	.23	-.16	.01
Black representation ratio	-.17	-.32	-.25	-.11
R^2	.78	.45	.66	.71

*Significant at the .05 level.
**Significant at the .01 level.

of minority employment, it may be the case that black elected officials are more effective in responding to constituent demands in unreformed cities.

In order to explore this possibility, I divided the entire sample into reformed and unreformed cities and conducted separate analyses of each group. An examination of the results reveals that the black council variable does, in fact, exert greater impact upon the level of minority employment in unreformed than in reformed governments. The coefficients for black council in unreformed communities are .31 for professionals and .23 for protective service positions (both of which are significant at the .01 level). The corresponding coefficients in reformed cities are only .09 and -.07.

The impact of unreformed structures upon enhancing the effectiveness of black public officials becomes dramatically apparent when we concentrate on cities with large black populations. An examination of Table 3 reveals that for unreformed cities with 10% or more black population the black council variable becomes the *most* significant determinant of minority employment for official-administrator and professional positions. With coefficients of .55 and .58, it is level of black representation on council rather than the black population variable that provides the dominant explanation in unreformed cities of why some governments hire more black decision makers and professionals than others. In reformed communities, however, black officials exert little effect upon minority employment success. In fact, for protective service positions the coefficient for the black council variable is actually negative (-.29).

Do these significant variations in black political effectiveness on the basis of differences in governmental structures also hold within individual regions? I divided southern, midwestern, and northeastern cities into reformed and unreformed categories and conducted separate analyses of each group. (The West was excluded from this process since only a few cities in that region have adopted unreformed structures.) An examination of the results reveals that unreformed political institutions significantly enhance the effectiveness of black political power. In the South, the regression coefficients in unreformed cities for the black council variable are .48 for officials-administrators, .40 for professional positions, and .29 for protective service jobs. In reformed cities the same coefficients are .13, -.06, and -.20 respectively. A similar though less consistent pattern emerges in the Midwest. In that region the coefficients in unreformed cities for black council are .60 for professional positions and .36 for protective service jobs. The corresponding relationships in reformed cities are only .18 and .03. In northeastern cities, however, governmental structure appears to exert little impact upon the effectiveness of minority political representation. The variability of effects across the various employment categories in the region does not support an unambiguous interpretation with respect to the role of local political institutions.

The conclusion to be drawn from these various analyses is that it is not sufficient for blacks to elect one or more of their number to city council. The governmental structure under which minority officials operate also makes a very considerable difference. Apparently, the greater sensitivity of unreformed governments to the diverse racial and class groups that populate the city translates into enhanced political effectiveness for the black community. In fact, the interaction between structure and representation exerts a major impact on minority job success in southern and midwestern cities. It will be recalled that no political effects were noted in the South and Midwest in the initial regional analysis. That finding was misleading. Politics makes a very real difference in *unreformed* southern and midwestern communities. But is there another explanation lurking beneath the surface of unreformed structures? To maintain that unreformed governments are simply more open and responsive to minorities is too imprecise a solution to be compelling. Perhaps a more satisfactory answer is to be found in the nature of local electoral systems.

Wards, At-large, and Black Political Effectiveness

A major component of the reformism index is the type of electoral arrangement. Reformed cities elect members of the council at-large while unreformed governments select these public officials from wards or districts. One major line of inquiry in the study of urban politics has examined the extent to which ward electoral systems enhance and at-large arrangements depress the level of black representation. Generally, the evidence supports the conclusion that minorities fare better in ward cities (Engstrom and McDonald 1981; Robinson and Dye 1978; Vedlitz and Johnson 1982). The extent to which wards actually contribute to favorable policy outcomes for blacks is an issue that has received much less scholarly attention. In general, the findings suggest that policy differences between ward and at-large cities are unlikely to be particularly dramatic (Helig and Mundt 1984). It should be pointed out, however, that no previous study has examined the effects of electoral arrangements upon employment outcomes.

In order to explore the possibility that there is an interactive effect between the nature of the electoral system and the effectiveness of black public officials, each of the 1,224 cities was classified into one of three groups: ward, at-large, or mixed. Again, separate analyses were conducted for each category. The results provide considerable support for the argument that minority elected officials are able to exert substantially more influence on employment outcomes in ward as opposed to at-large cities. In cities with ward systems the coefficients for the black council variable are .18 for total employment, .34 for professional positions, and .23 for protective service jobs (all significant at the .01 level). However, the corresponding coefficients in at-large cities are .00, .05, and −.10 respectively. As might be expected, cities with both ward and at-large council seats occupy the middle ground. The coefficients in those cities for total jobs, professionals, and protective service positions are .06, .27, and .11 respectively. The only exception to the pattern of

Table 4. Multiple Regression of Black Professionals by Region

Variable	South Ward	South At-Large	South Mixed	West[a] Ward	West[a] At-Large
Total population	-.01	.11	.31	.12	-.05
Percentage black population	.45**	.72**	.48**	.81**	.42**
Black mayor	.01	.13*	.10	.00	.03
Percentage black council	.48*	-.16	.27	-.27*	.53**
Percentage workforce clerical and service	.02	.02	.10*	.04	.10
Income-housing-education index	.05	.17*	-.13	.05	.02
Black representation ratio	-.13	.11	-.06	.26*	-.12*
R^2	.56	.47	.75	.86	.77

[a] There were too few mixed cities in the West to make the regression analysis meaningful.

*Significant at .05 level.

**Significant at .01 level.

greater minority influence in ward cities occurs for official-administrator positions. In that instance the largest coefficient is noted for cities with mixed arrangements (.28). An examination of the data, however, reveals that black council members in mixed systems tend to be elected from the ward rather than the at-large seats. Consequently, the finding of enhanced black power in ward systems is reinforced.

The fitting of the next piece of the puzzle revolves around the issue of regional differences. Does the interaction between black public officials and electoral type hold within regions? Again, the sample of cities was classified according to regional variations and further categorized on the basis of ward, at-large, and mixed systems. Then separate analyses were performed for each of the three groups within each of the four regions. An examination of the results continues to provide strong support for the argument that wards enhance minority political effectiveness while at-large arrangements stymie black efforts to influence employment outcomes. This pattern is particularly evident in southern cities. Tables 4–5 demonstrate that election in a ward system in the South significantly increases black political power for professional and protective service positions. The coefficients for the black council variable and professionals are .48 for wards, .27 for mixed, and -.16 for at-large systems, while the corresponding coefficients for protective service jobs are .34 for wards, .06 for mixed and -.24 for at-large. A similar pattern is also noted for officials-administrators. The coefficient in southern cities for the black council variable and decision-making positions is .42.

Clearly, then, it makes a very considerable difference in southern cities whether black public officials are elected from wards or at-large. Ward arrangements dramatically enhance minority political effectiveness while at-large systems seriously damage the ability of black members of the council to deliver tangible, material rewards to their constituents. The importance of the ward system to black effectiveness is demonstrated by the fact that for decision-making and professional positions the black council variable in ward cities becomes one of the dominant factors in the regression models. With coefficients of .42 and .48, it actually rivals the significance of the black population variable in terms of accounting for variation in employment outcomes.

However, the finding that ward systems are crucial to black employment suc-

Table 5. Multiple Regression of Black Protective Service Positions by Region

Variable	South Ward	South At-Large	South Mixed	West[a] Ward	West[a] At-Large
Total population	-.10	-.03	.19	.12	.00
Percentage black population	.77**	.85**	.83**	.82**	.42**
Black mayor	.00	.15**	.00	.00	.15*
Percentage black council	.34*	-.24**	.06	-.07	.45**
Percentage workforce clerical and service	.00	.01	-.08	-.06	.08*
Income-housing-education index	.09	.14**	-.17	-.05	.05
Black representation ratio	-.13	.10	-.05	.08	-.12*
R^2	.82	.72	.78	.84	.80

[a] There were too few mixed cities in the West to make the regression analysis meaningful.

*Significant at .05 level.

**Significant at .01 level.

cess in the South does not hold at all in western cities. Further examination of Tables 4-5 reveals, in fact, that the reverse is true. The relationships for the black council variable and the employment categories in the West are consistently negative in ward cities with coefficients of -.27 for professionals, -.07 for protective service positions, and -.11 for officials-administrators. Instead, it is at-large electoral arrangements that dramatically boost black employment fortunes. The coefficients for the black council variable in at-large western cities are .53 for professionals, .45 for protective service positions, and .59 for officials-administrators. In fact, the black power variable becomes the key factor in all three regression equations and exceeds the significance of black population in each instance. For example, the coefficient for the black council variable for officials-administrators is .59, while the coefficient for black population is only .31.

Thus, governmental structure makes a very substantial difference. The nature of local political institutions and the type of electoral arrangement do matter and in fact matter a great deal. However, neither form of government or electoral system exerts an independent impact upon black employment patterns. Instead, structural arrangements and political institutions operate to either enhance or limit the effectiveness of black political leaders. Significantly, the substantial impact of these institutional conditions also holds across regions.

But why do electoral systems exert a differential impact on black political effectiveness in southern and western cities? Why do ward systems enhance black power in the South but detract from it in the West? Our efforts at developing a theory to account for these differences should begin with a consideration of the work of Browning, Marshall, and Tabb (1984). In their study of 10 California cities they concluded that black political effectiveness was a direct function of membership in the dominant political coalition. Black members of the council who were part of the dominant coalition in city government exerted a significant impact upon minority employment policy. Exclusion from the political coalition that controlled public policy severely retarded the ability of blacks on council to influence outcomes.

The concept of dominant political coalitions is an important contribution to the study of minority employment policy.

However, two important questions remain unanswered. First, is black membership in the dominant coalition a realistic possibility in southern cities? Second, what is the relationship between electoral system and the likelihood of building coalitions that include minorities? The 10 cities in the Browning, Marshall, and Tabb study are not representative of all cities in the country. One important dimension on which cities differ is the extent of residential segregation and the likelihood of racial polarization. The interactive effect between racial segregation, electoral system, and coalition building may be crucial.

Vedlitz and Johnson (1982, 734) find ward systems have a major impact upon the election of blacks to council in segregated cities. In fact, they find that "in the segregated cities black representation in single-member district communities was nearly three times more favorable than in at-large communities." However, the type of electoral system makes no difference in nonsegregated cities with respect to black representation on council. Vedlitz and Johnson offer two explanations for this finding. First, racial segregation may also imply racial polarization in some cities. Such conflict may limit the likelihood of black electoral success in at-large systems. Second, at-large arrangements may require blacks to form coalitions with whites and building such coalitions will be difficult in segregated cities.

The conclusions reached in these two studies are relevant to the issue at hand. However, both a considerable refinement and an extention of the findings are in order. Clearly, coalition building should prove to be a more difficult task in segregated than in nonsegregated communities. In addition, the political effectiveness of blacks is likely to vary on the basis of two dimensions: segregation and the nature of the electoral system. In segregated cities blacks will have little opportunity to join the dominant coalition in either ward or at-large communities. Since the white majority will be unwilling to share substantial power with them, blacks will be forced to rely upon other means to influence policy outcomes. These alternative methods may include attempts to develop alliances with other minorities and disaffected white liberals as well as efforts to increase the minority share of council seats. In segregated and racially polarized communities black political power will prove to be the most effective in ward systems.

Minorities elected under at-large arrangements in segregated cities will exert little influence upon policy because they will be excluded from the dominant political coalition. In addition, they will have little opportunity to explore alternative means of enhancing their power because at-large systems in segregated communities severely depress the election of minorities.

Differences are also likely to exist between ward and at-large cities with respect to the types of coalitions formed and the style of black leaders elected. The coalitions built in segregated communities under at-large arrangements are not apt to survive the electoral stage. Also, blacks recruited to run as coalition candidates in at-large contests must be acceptable to the white majority. Consequently, it is unlikely that such candidates will vigorously champion a redistributive agenda on behalf of their constituents. On the other hand, blacks elected from wards in racially polarized cities will almost certainly take office with a clearly defined set of policy objectives. As a result, one can expect that in segregated communities blacks elected from wards will exert a greater influence on employment outcomes than leaders selected at-large.

In nonsegregated cities, however, the ward system may actually work to the disadvantage of blacks. In communities where little racial polarization exists, blacks elected from wards may have no incentive to engage in coalition building. As a result, they may be ignored by the dominant coalition. Black members of the city council in nonsegregated communities will experience their greatest success in terms of influencing employment outcomes in at-large systems where exclusion is not a goal of the dominant majority but where coalition building is nonetheless essential to electoral victory.

This interpretation of the interaction between black political effectiveness, segregation, and electoral system is highly consistent with the data. Although segregation indexes were not available for each city, I was able to obtain such data for the major cities in the sample (Vedlitz and Johnson 1982). I relied on these indexes of residential segregation to calculate a mean segregation score for each region. An examination of the results reveals that the South is the most segregated region in the country, followed by the Midwest, with the West and the Northeast ranking as the least segregated. As we have seen, black political power in segregated southern communities is much more effective in ward systems than under at-large arrangements, while in the West the reverse is the case. Black influence upon employment policy is dramatically enhanced in at-large western cities but severely limited in ward systems. The logical explanation is that blacks in segregated southern cities have little opportunity to effectively participate in the dominant coalition that controls public policy in either ward or at-large systems. Consequently, ward structures enhance black political effectiveness because they significantly improve the prospects of minority electoral success. Once elected in ward cities black officials can then use other means to influence policy choices (forming coalitions with other minorities and disaffected whites). In at-large segregated cities, however, blacks on council exert no influence on employment patterns. Not only do at-large systems in segregated communities severely depress the level of black representation on council, they also attract black candidates who, while acceptable to the white majority, are unlikely to press for a redistributive agenda once elected to office.

In less segregated cities, however, at-large systems appear to operate to the advantage of blacks. In the West, black political power is much more effective in at-large than in ward cities. According to our interpretation, less segregated communities are also less racially polarized. Therefore, the dominant majority has relatively little incentive to engage in a politics of minority exclusion. Blacks elected under at-large systems will be more likely to gain access to the dominant coalition because of the need to attract communitywide support during the electoral contest. On the other hand, a ward structure may tend to isolate black officials in nonsegregated cities and reduce their political influence and policy effectiveness. Since there are few incentives to build electoral coalitions in ward systems, the likelihood of joining the dominant policy coalition once elected to office may be greatly reduced.

Hispanic Employment

What role does politics play in determining employment outcomes for Hispanics? An examination of Table 6 reveals that for the entire sample of 1,224 cities political factors are more significant for Hispanics than for blacks. Although Hispanic mayors are not particularly important (the largest mayoral coefficients are only .12 and .13), the Hispanic council variable exerts a consistent if moderate effect across all employment categories. The coefficients for Hispanic council for all cities are .21 for total employment, .29 for officials-administrators, .20 for professionals, and .19 for protective service positions. As was the case for blacks, however, the minority population varia-

Table 6. Multiple Regression of Hispanic Employment Categories, All Cities and Cities 10% or More Hispanic

	Total Jobs		Officials-Administrators		Professionals		Protective Service Workers	
Variable	All Cities	10% or More	All Cities	10% or More	All Cities	10% or More	All Cities	10% or More
Total population	–.03	–.05	.05*	–.08	–.04	–.05	–.05**	–.11
Percentage Hispanic population	.79**	.65**	.55**	.50**	.57**	.47**	.72**	.65**
Hispanic mayor	.04**	.06	.12**	.12*	.13**	.13*	.09**	.09
Percentage Hispanic council	.15**	.21**	.29**	.40**	.20**	.36**	.19**	.31**
Percentage workforce clerical and service	.03**	.08*	.00	.02	–.03	–.09	.00	–.01
Reform index	.01	.01	.00	–.05	.00	.03	.00	.00
Income-housing-education index	.07**	.04	.04	.06	.05	.00	.07**	.12*
Percentage black workers	–.05**	–.06	–.03	.00	–.02	.00	.00	.04
Hispanic representation ratio	–.02*	–.06	–.06**	–.19**	–.05*	–.23**	–.04**	–.16**
R^2	.87	.84	.69	.72	.61	.63	.81	.78

*Significant at .05 level.
**Significant at .01 level.

ble provides the dominant explanation of the variation in employment patterns. The coefficients for the percent Hispanic population variable and the four employment categories are .79, .55, .57, and .72 respectively.

Hispanic councils exert an even greater impact on employment outcomes in cities with a substantial Hispanic population. A further examination of Table 6 reveals that in cities with 10% or more Hispanics the coefficients are .21 for total employment, .40 for officials-administrators, .36 for professionals, and .31 for protective service workers. Again, the relationships between political power and employment patterns are considerably stronger for Hispanics than for blacks.

In general, the analysis supports the conclusion that the determinants of the variation in public employment are similar for both minority groups. The size of the minority community is the best predictor of how well blacks and Hispanics will fare in the distributional struggle over finite resources. None of the other demographic variables makes a difference. However, an exception to the observation that minority employment outcomes can be traced to the same set of causal factors is the finding that Hispanic councils are considerably and consistently more influential than black councils. This differential impact is particularly pronounced in cities with a substantial minority population. It should also be noted that minority employment does not appear to be a zero-sum game for blacks and Hispanics. It is not the case that Hispanics fare poorly in those cities where blacks hold a sizable share of the municipal jobs.

What impact do regional distinctions have upon Hispanic employment patterns? Since northeastern and midwestern cities have small Hispanic populations, these two regions were eliminated from the analysis. I will focus on the South and West. Further, the states of Cali-

Table 7. Multiple Regression of Hispanic Officials-Administrators by Region

	Southwest		South		West	
Variable	All Cities	10% or More	All Cities	10% or More	All Cities	10% or More
Total population	-.11*	-.08	-.04	-.11	-.15*	-.13
Percentage Hispanic population	.45**	.46**	.77**	.85**	.39**	.27**
Hispanic mayor	.15**	.15**	.13**	.12	.09	.12
Percentage Hispanic council	.44**	.52**	.10	.11	.42**	.69**
Percentage workforce clerical and service	.04	.03	-.02	-.03	.05	.07
Reform index	-.04	-.05	-.03	-.03	.00	-.04
Income-housing-education index	.07	.05	-.03	.04	.13*	.06
Percentage black workers	-.02	.00	.00	.11	.03	.02
Hispanic representation ratio	-.17*	-.25**	-.09**	-.16	.05	-.30*
R^2	.72	.76	.82	.85	.56	.60

*Significant at .05 level.
**Significant at .01 level.

fornia, Texas, New Mexico, and Arizona were combined to form a southwest region. An examination of the results reveals that the Hispanic council variable is crucial to the employment success of Hispanics in southwestern and western cities. This finding is particularly pronounced in cities with substantial Hispanic populations. For southwestern cities with 10% or more Hispanics the regression coefficients for the council variable are .31 for total employment, .52 for officials-administrators, .49 for professionals, and .42 for protective service positions. In fact, for decision-making and professional positions (Tables 7-8) the proportion of council seats controlled by Hispanics exceeds the significance of the size of the Hispanic population in determining employment outcomes. Again, however, Hispanic mayors are not particularly influential. The coefficients for the mayoral variable are .08, .15, .16, and .12 respectively.

Table 8. Multiple Regression of Hispanic Professionals by Region

	Southwest		South		West	
Variable	All Cities	10% or More	All Cities	10% or More	All Cities	10% or More
Total population	-.10	-.03	-.06	-.11	-.09	.02
Percentage Hispanic population	.48**	.42**	.78**	.90**	.41**	.18
Hispanic mayor	.15**	.16**	.04	-.10	.15**	.22**
Percentage Hispanic council	.36**	.49**	.14	.30	.24**	.68**
Percentage workforce clerical and service	-.05	-.06	-.07**	-.25**	-.01	.03
Reform index	.03	.04	-.01	.14	.05	.08
Income-housing-education index	.08	-.01	.00	.08	.09	-.10
Percentage black workers	.00	.00	.03	.21*	.03	.00
Hispanic representation ratio	-.16*	-.29**	-.08*	-.25	-.04	-.45**
R^2	.62	.66	.77	.79	.40	.49

*Significant at .05 level.
**Significant at .01 level.

An examination of western cities reveals that the Hispanic share of council seats is similarly crucial to minority employment success. In cities with 10% or more Hispanics, the coefficients for the council variable are .40 for total employment, .69 for officials-administrators, .68 for professionals, and .40 for protective service positions. Again, politics is more important than demography with respect to accounting for the variation in decision-making and professional positions. The coefficients for Hispanic council for these employment categories are .69 and .68, while the coefficients for the size of the Hispanic population are only .27 and .18. Clearly, Hispanic political power as measured by the minority share of council seats is the dominant influence in determining how well the Hispanic community fares in the struggle for key decision-making and professional positions in city government. Politics counts in southwestern and western cities and it counts heavily.

A different story can be told for southern cities. Neither the Hispanic mayor nor Hispanic council variables make much difference in either the full sample or in those cities with at least 10% Hispanic population. The largest coefficient to achieve statistical significance is noted for the relationship between Hispanic council and protective service positions (.20). It should be pointed out, however, that with the exception of Texas and Florida, southern cities have relatively small Hispanic populations. The fact remains that in those cities with substantial numbers of Hispanics (cities which are disproportionately concentrated in the West and Southwest) political power provides a powerful explanation of the variation in distributional outcomes.

For blacks the structural and institutional characteristics of local governments significantly enhance—or limit—the effectiveness of minority political power. Further, the interaction between structure and political power varies dramatically on a regional basis. This differential impact could be attributed to the level of residential segregation in a community. It is reasonable to expect that a similar causal relationship is present with respect to Hispanic employment patterns. Unfortunately, the small sample sizes preclude the possibility of examining the interaction between governmental structure, segregation, and political effectiveness at the regional level. Although analyses were conducted for individual regions, the volatility of the regression coefficients does not allow for any confidence in the results. Therefore, I will limit the discussion of the findings to an analysis of all cities.

It will be recalled that for blacks the ward system enhanced political effectiveness in segregated cities but seriously weakened it in at-large cities. The reverse was the case in nonsegregated communities where at-large arrangements dramatically strengthened the influence of black political leaders but where wards isolated minority officials and retarded their effectiveness. Can we expect an identical set of findings for Hispanics? Probably not. Although data are not available on the extent of Hispanic residential segregation for cities in the sample, there is considerable evidence to support the conclusion that Hispanics are less segregated than blacks. In fact, Anglo-Hispanic groupings exhibit the lowest levels of residential segregation while Anglo-black groupings show the highest (Hwang and Murdock 1982; Lopez 1981; Moore and Mittelbach 1966). Lopez (1981, 55) analyzed changes in segregation levels in 56 southwestern cities between 1960 and 1970 and found that "the difference in Mexican-American-Anglo segregation has narrowed while Mexican-American-black segregation has become more pronounced. It would appear that the segregation of Mexican-Americans from Anglos is becoming more like that of European ethnics . . . and less like that of blacks."

Not only are Hispanics less segregated

than blacks, but the perceived social distance between Hispanics and Anglos is less than it is between blacks and whites. Anglos have a greater tolerance toward Hispanics than toward blacks (Davidson and Gaitz 1973; Pickney 1970), and Hispanics are more frequently thought of as white by Anglos (Lopez 1981). Given the reduced levels of segregation for Hispanics and the greater acceptance of Hispanics by Anglos, we should expect that the interaction between electoral system and residential segregation will differ for each minority group. Specifically, Hispanic political power should be most effective under at-large arrangements and least influential in ward systems. This assumption is based upon the finding from the black analysis that at-large electoral structures in nonsegregated cities visibly strengthen minority political effectiveness while ward arrangements in nonsegregated cities severely limit that effectiveness.

When all 1,224 cities are classified according to ward and at-large systems and separate analyses are conducted of each, in fact, at-large electoral structures do significantly enhance Hispanic political effectiveness. An examination of Table 9 reveals that in every employment category the Hispanic council variable exerts a much greater influence in at-large than in ward cities. The coefficients for Hispanic council in at-large communities are .27 for total employment, .34 for officials-administrators, .30 for professionals, and .27 for protective service positions. The corresponding coefficients in ward systems are only .04, -.11, -.29, and .11 respectively.

Implications

The research reported here significantly advances the debate concerning the impact of local political institutions upon policy outcomes. Political structure does make a difference. Sometimes, in fact, it becomes the decisive factor in determining winners and losers in the distributional struggle. It should be emphasized, however, that the nature and characteristics of local political institutions do not directly influence employment patterns. Instead, they operate to create an environment which serves to either enhance or retard the effectiveness of minority political power. These findings challenge the assumption that electoral coalitions and election outcomes do not really matter, that political institutions (form of government, electoral system) are only historical curiosities with little policy relevance, and that city councils as decision-making bodies are inactive at worst or reactive at best. Minority political efforts can pay substantial dividends. Blacks and Hispanics are not helpless. The election of black and Hispanic representatives to the city council can produce more and better jobs for the minority community. However, that effectiveness is mediated by a powerful set of factors that includes the form of government, the level of racial segregation, and the nature of the electoral system.

The finding that minority mayors are unable to influence employment policy but that both black and Hispanic representatives on city council exert a major impact upon employment patterns also deserves comment. Traditionally, minority mayors have been perceived to be in the best position to reward their constituents. In some cities, at least, they appoint personnel directors, affirmative action officers, and department heads. They have the potential to set the tone of government and influence the administration and implementation of personnel policy. But there is reason to doubt the conventional wisdom. Karnig and Welch (1980) observe that black, as well as white, mayors are limited by the fiscal condition of the city and by the federal and state roles. In addition, black mayors tend to represent cities in the worst financial condition. Finally, whites will oppose attempts on the part of minority mayors to

Table 9. Multiple Regression of Hispanic Employment Categories by Electoral System

Variable	Total Jobs			Officials-Administrators			Professionals			Protective Service Workers		
	Ward	At-Large	Mixed	Ward	At-Large	Mixed	Ward	At-Large	Mixed	Ward	At-Large	Mixed
Total population	-.01	-.02	-.10*	-.04	-.04	.00	-.01	-.01	-.07	-.02	-.02	-.10
Percentage Hispanic population	.91**	.69**	.87**	.77**	.38**	.51**	.80**	.39**	.62**	.86**	.63**	.84**
Hispanic mayor	—	.04	.09**	—	.28**	-.13**	—	.24**	-.07*	—	.14**	.02
Percentage Hispanic council	.04	.27**	-.04	-.11**	.34**	.53**	-.29	.30**	.40**	.11**	.27**	.12*
Percentage workforce clerical and service	.01	.03	.06	-.01	.02	.04	-.04	.03	.02	-.02	.00	.05
Income-housing-education index	-.06**	.09**	.10**	-.01	.06	.00	-.05	.07	.07	-.01	.09**	.10*
Percentage black workers	-.03	-.04*	-.07*	-.03	.00	-.08	-.03	.00	-.03	.00	.00	-.03
Hispanic representation ratio	-.02	-.05**	-.02	.05	-.09**	-.16**	.09**	-.09**	-.14**	-.03	-.08**	-.04
R^2	.92	.87	.93	.80	.71	.85	.75	.61	.82	.88	.81	.87

*Significant at .05 level.
**Significant at .01 level.

substantially redistribute resources. To this I might add that black mayors will likely find it necessary to court a citywide constituency and may find it exceedingly difficult to respond to the particularistic demands of the minority community.

On the other hand, the literature has not been kind to city councils. The research on municipal budgeting paints a picture of a council eager to defer to the expertise of the executive and the bureaucrat, while the recruitment literature (Prewitt 1970) reinforces the images of council members as motivated by a sense of civic obligation and "voluntarism." Successful candidates assume office without the benefit of a clearly defined policy agenda. Consequently, they are easy prey for the expert and experienced mayor, city manager, and administrator. Typically, city councils are described as reactive institutions characterized by high turnover and inadequate staff support. Individual council members are poorly paid and inexperienced, devote only a few hours a week to the job, take office without clear policy goals, are little interested in staying in office, and are no match for professional politicians, managers, and administrators.

However, this bleak picture of the policy role of city councils is likely overdrawn. First, these conclusions are generally based on findings from case studies of only one or a few cities and may not be representative of all communities. Second, the available research has largely ignored minority council members who are apt to be much more diligent in their pursuit of redistributive policy objectives than white businessmen who seek office out of a sense of civic obligation. Third, the literature has generally failed to distinguish between ward and at-large councils. We have seen that ward systems significantly enhance the effectiveness of black political power in segregated communities while at-large arrangements boost minority influence in nonsegregated cities. It is probable that the behavior, activities, and influence of council members with respect to other policy agendas also varies significantly on the basis of electoral system differences. Studies that have ignored the substantial impact of these differences on council behavior and effectiveness may have produced a distorted picture of the policy role of local legislatures.

In fact, several recent studies suggest that city councils may exert a greater influence upon policy outcomes than was previously thought to be the case. Abney and Lauth (1986, 188) in a survey of department heads in cities over 50 thousand in population find that 32% of department heads "perceived contacts by council members on behalf of constituents to result in a distortion of their priorities." Thirty-seven percent reported that "requests from council members are important in their decisions about the pattern of service delivery," and 42% of the department heads in reformed cities ranked the council as having the *most* impact on their departments when compared to the chief executive and interest groups. Thomas Baylis (1983) studied the San Antonio council and finds that in addition to the selection of the city manager and the approval of the budget, the council exerted significant influence on the allocation of federal funds and on appointments to major municipal boards and commissions. Dye and Renick (1981) analyze employment patterns in 42 cities and conclude that the level of minority representation on council was the most significant determinant of the variation in minority employment. Finally Browning, Marshall, and Tabb (1984) in their study of 10 California cities find that the primary explanation of differences in minority share of public jobs is the extent to which minority council members have gained entry to the dominant political coalition.

Although the research literature does not allow us to resurrect the city council as the dominant policy-making institution, the evidence is sufficiently extensive to cast doubt on conventional wisdom. Councils are something more than reactive bodies manipulated at will by executives and bureaucrats and disinclined to press their own policy agendas. At a minimum, the research reported here, as well as the work of others (Browning, Marshall, and Tabb 1984; Dye and Renick 1981) reveals that minority council members play a crucial role with respect to employment policy.

Another point deserves consideration. Why does the level of Hispanic political representation exert a greater impact upon employment than black representation? Although we do not have data on the variety of factors that might account for this differential effectiveness, I can suggest one possibility. Browning, Marshall, and Tabb (1984) argue that one factor that will cause minority officials to take action with respect to employment policy is the severity of the problem; that is, minority council members are more likely to press for a redistribution of resources in cities where the minority share of the workforce lags far behind the minority proportion of the population. If this interpretation is correct, we should expect that the gap between share of the municipal workforce and proportion of the population will be greater for Hispanics than for blacks. Parity ratios were calculated by dividing the percentage black or Hispanic in each of the four employment categories by the minority share of the total population. Higher scores indicate a move toward parity while declining scores suggest an underrepresentation of minorities. An examination of the results reveals that while both blacks and Hispanics are underrepresented in terms of their share of municipal jobs, the parity gap is considerably greater for Hispanics than for blacks. The ratios for blacks are .52 for officials-administrators, .76 for professionals, and .73 for protective service positions. However, the corresponding scores for Hispanics are only .36, .45, and .58 respectively. If minority elected officials are moved to act on the basis of their perception of the severity of the problem, we can conclude that Hispanic council members will more vigorously press for a redistribution of public jobs. Although alternative explanations need to be explored, this interpretation is certainly consistent with the data. Hispanic representatives exert a greater impact upon employment policy because the gap between the Hispanic share of the workforce and their proportion of the total population is greater than it is for blacks.

Conclusion

I return to the original question: Does the political process work for blacks and Hispanics? The answer is *yes*—under certain conditions. Racial minorities are not helpless in their quest for more of the public benefits city government has within its power to withhold or bestow. Their fate does not depend completely upon the largess of the federal government or the sympathy and support of white allies. They are not captives of powerful forces beyond their ability to shape and control. If blacks and Hispanics organize, compete in the electoral arena, and elect one or more of their number to city council, they can lay claim to a larger slice of the public pie. In the process, however, minority influence is powerfully constrained by local political institutions.

When all cities were analyzed, the results reveal that the size of the minority population rather than minority political power provides the dominant explanation of why some municipal governments hire many more blacks and Hispanics than others. It is when we turned to an analysis of employment patterns on a regional basis that we discovered very substantial differences in the effectiveness of minority political representation. Region, in turn, was found to mask significant variations both within and across regions with respect to the influence of form of government and electoral system.

For example, when all southern cities were analyzed, we saw that black elected officials exert little impact upon employment outcomes. If we had left the matter at that, the inescapable conclusion would have been that politics doesn't make a difference in the South. However, further examination of the data revealed a powerful interactive effect between form of government, electoral system, and black political effectiveness. When southern communities were divided into reformed and unreformed governments we saw that black council members were much more effective in terms of influencing employment outcomes in unreformed cities. The evidence supports the argument that unreformed political institutions are more open and responsive to the diverse groups within the city. In addition, hiring decisions are more apt to be sensitive to the demands of elected officials in unreformed communities, while personnel matters in reformed governments will likely be controlled by the city manager and professional staff.

The nature of the electoral system was also found to be of major significance. In southern cities black political effectiveness was greatly enhanced by ward arrangements and severely retarded by at-large systems. The reverse was the case in the West where at-large elections provided a powerful boost to black influence and ward arrangements imposed significant limits on the effectiveness of minority council members. In both regions the political structure variable exerted a major and sometimes even dominant influence upon minority employment levels.

The failure in past research to examine the interaction between residential segregation, electoral system, and minority political effectiveness concealed the powerful role played by black and Hispanic council members. It is not that politics is of little significance in arraying winners and losers in the distributional contest. Rather, minority political representation makes absolutely no difference under some institutional arrangements but is of decisive importance under others.

I also suggest that a critical reevaluation of the respective policy roles of the mayor and city council is in order. Traditionally, councils have been perceived as reactive bodies easily manipulated by the executive and quick to defer to the expertise of the professional administrator. This perspective holds that only a strong, aggressive mayor has both the vision and requisite political skills and resources to confer significant public benefits upon the minority community. However, the findings reported in this paper reveal that minority council members rather than mayors are the key actors in the policy process. It is important to point out once again, however, that the nature of the local governmental and electoral system is crucial to both enhancing and limiting that effectiveness. The influence of minority council members is conditioned and mediated by a complex of factors that includes the extent of racial polarization and the type of structural arrangement.

Notes

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1. Most of the large cities with black mayors are found in the sample. These include Philadelphia, Atlanta, New Orleans, Detroit, Chicago, Los Angeles, Newark, and Richmond. The mean percentage black in all the black mayor cities was 43.0%.

2. *Officials and Administrators*. Occupations in which employees set broad policies and exercise overall responsibility for execution of these policies or direct individual departments or special phases of the agency's operations. Includes department heads, bureau chiefs, division chiefs, directors, deputy

directors. *Professionals.* Occupations requiring specialized and theoretical knowledge usually acquired through college training or work experience. Includes personnel and labor relations workers, social workers, doctors, economists, lawyers, engineers, teachers, accountants, police and fire captains, and lieutenants. *Protective Service Workers.* Occupations in which workers are entrusted with public safety and security. Includes police officers, fire fighters, guards, correctional officers, detectives. *Service-Maintenance Workers.* Includes refuse collectors, construction laborers, bus drivers, custodial personnel, grounds keepers.

3. The regions were composed of the following states: Northeast (Connecticut, Maine, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, Vermont); Midwest (Illinois, Indiana, Iowa, Kansas, Michigan, Minnesota, Missouri, Nebraska, North Dakota, Ohio, South Dakota, Wisconsin); South (Alabama, Arkansas, Delaware, District of Columbia, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, West Virginia); West (Alaska, Arizona, California, Colorado, Hawaii, Idaho, Montana, Nevada, New Mexico, Oregon, Utah, Washington, Wyoming).

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Kenneth R. Mladenka is Associate Professor of Political Science, Texas A&M University, College Station, TX 77843-4348.

DISTRIBUTIVE POLITICS AND THE ALLOCATION OF FEDERAL GRANTS

MICHAEL J. RICH
Brown University

Understanding the dynamics of policy distribution requires an appreciation of federal grant programs that have achieved a prominent place in nearly all areas of domestic policy. The theoretical literature on distributive politics, however, focuses almost exclusively on a centralized, top-down view of policy distribution. By examining the role of presidents, legislators, and bureaucrats, scholars have ignored participants who have become key actors in the distribution of federal expenditures—the recipient jurisdictions. This analysis of the allocation patterns under six federal programs shows that local governments exert important influences on the distribution of federal grants and that the distributional patterns and their determinants vary over time. The analysis also points out the importance of disaggregation by focusing on programs and recipient jurisdictions, as opposed to total federal expenditures and regions, states, or congressional districts.

Each year the federal government allocates billions of dollars in benefits to individuals, private and nonprofit corporations, and state and local governments. The form of these benefits varies enormously: they include food stamps, housing vouchers, and income supplements to the poor; tax credits for investments in plant and machinery made by business and industry; and a variety of grant-in-aid programs to state and local governments—to name but a few.

Despite the amount and variety of government benefits, political scientists have devoted comparatively little attention to investigating their distributional patterns as compared to such things as the voting behavior of citizens and legislators. There is, however, a growing literature on the distribution of government benefits and while we have developed a greater appreciation for their size, scope, and variety (Page 1983), our understanding of how and why they are distributed warrants further empirical investigation and theoretical refinement. Several studies have examined the politics of policy distribution, but they have focused on different types of government benefits (e.g., taxes, federal facilities and employment, and grants-in-aid) and the equivocalness of the empirical findings prevents generalizations either across or within policy instruments.

The incidence of discrepant research findings is especially great among studies of federal grant programs, which have comprised an increasing share of federal domestic spending in the post-World War II era. In 1950, federal grants totaled only $2.2 billion and represented less than 10% of federal domestic outlays. By 1980 federal grants to state and local governments had grown to $82.9 billion, equivalent to one-fifth of federal domestic outlays and nearly one-fourth of state and local government expenditures. Despite President Reagan's efforts to cut domestic spending, federal grants exceeded $100 billion in fiscal year 1985.

Much of the ambiguity in the empirical literature on the distribution of federal grants can be attributed to the poor fit between the conceptual models used to examine the politics of policy distribution and the phenomenon investigated. Few studies analyze the distribution of government benefits from an intergovernmental perspective. Indeed, most studies of the distribution of federal funds tend to emphasize the role of national political institutions (that is, Congress and the bureaucracy) and underplay the role of state and local governments. Examination of many federal programs, however, reveals that recipient jurisdictions play an active role in determining the distribution of federal aid. In addition, most studies express the dependent variable as total federal, functional, or departmental expenditures when the more appropriate measure is program allocations. The politics of policy distribution focuses on programs—not departments, functional areas, or total federal spending. Further, empirical studies of federal grant programs generally employ static research designs; that is, many studies only examine allocations for a single year, others aggregate allocations over several years into a measure of total program funding. Few studies examine grant allocations over multiple time points during a program's history.

Since federal grant programs have achieved such a prominent place in nearly all domestic policy areas, understanding the dynamics of policy distribution requires an appreciation of the politics of federal programs. My central objectives are threefold: (1) to show that local governments exert important influences on the distribution of federal expenditures; (2) to demonstrate the importance of disaggregation for both grantors (federal program allocations—not total, functional, or departmental expenditures) and grantees (cities—not regions, states, or congressional districts); and (3) to illustrate the dynamics of policy distribution by analyzing allocation patterns and their determinants over time. These points will be made through an analysis of the distribution of federal grants for community and economic development among cities with populations of 50 thousand or more during the period 1950–84, a time frame that extends from the beginning of the Urban Renewal program through the end of the first term of the Reagan administration. Community and economic development is an appropriate policy area for studying the politics of federal aid distribution because it captures many of the overall changes that were taking place in the federal system during this period.[1] In addition, throughout the four decades of federal involvement in this policy area, distributional issues have played a significant role in the debate surrounding the creation and reauthorization of federal grant programs.

Theories of Policy Distribution

Several theories have been offered concerning the distribution of government benefits. Four have been prominent in the literature and are briefly described below.

Political-Business Cycles

One theory of the distribution of government benefits promotes the notion of a political-business cycle in which government benefits tend to peak during those periods immediately preceding national elections. While scholars have tracked the linkages between elections and economic performance in areas such as inflation, unemployment, and interest rates (e.g., Beck 1987; Hibbs 1977; Nordhaus 1975), studies have also shown that such increases in transfer payments as social security and veterans benefits also fluctuate with the electoral cycle (Tufte

1978). Others have found similar results for the distribution of federal grants to state governments (Archer 1982; Cingranelli 1983; Johnston 1978, 1979; Reid 1980). Another group of scholars examined political-economic cycles and the content of federal urban policy (Friedland and Wong 1983; Mollenkopf 1983; Owen 1983; and Piven and Cloward 1971) and found that Democratic administrations tend to use federal urban programs to reward and nurture their big city constituencies, whereas Republican administrations prefer programs that devote a greater emphasis to smaller and relatively better-off (suburban) cities.

This view of policy distribution, however, is oversimplified. For most government benefits, Congress must legislate changes in program structure, including, at a minimum, eligibility criteria and allocational mechanisms. While presidents may articulate a new policy, one that may more closely fit the interests of particular constituency groups, the president must convince Congress to accept this policy change. In addition, even when presidents do succeed in persuading Congress to adopt policy changes, those changes may have little impact on the decision rules used by bureaucrats in allocating program funds. The empirical literature on the distribution of urban services (Jones 1980; Levy, Meltsner, and Wildavsky 1974; Lineberry 1977; Mladenka 1980) quite clearly illustrates the important role bureaucracies play in allocating government benefits. While differences in the types of benefits distributed by federal and local bureaucracies may not make studies of the two directly comparable, few would take exception with the notion that bureaucracies exercise considerable discretion in allocating government benefits.

Furthermore, findings from recent empirical studies suggest that the electoral-spatial linkage regarding the distribution of government expenditures is not as strong as some have argued. Anagnoson (1982) found that while some intervention from the White House and Capitol Hill occurred in the two programs he examined, they involved projects that would have been funded anyway and affected only the timing of the decision, not the decision to fund the project. Feldman and Jondrow reported that changes in federal spending (increases or decreases) had no effect on the share of the vote going to House incumbents seeking reelection: "All in all, we found *no* evidence that obtaining local federal spending for his district or protecting against spending cutbacks is a useful way for a congressman to pursue reelection" (1984, 155).

Pork Barrel Politics

Much of the theoretical and empirical work by political scientists on policy distribution has focused on Congress and its penchant for pork barrel politics. Models of congressional behavior have emphasized the importance of the norms of universalism and reciprocity (Fiorina 1981; Mayhew 1974; Shepsle and Weingast 1981; Weingast, Shepsle, and Johnsen 1981) in legislative decision making for what Lowi (1964, 690) calls distributive policies, policies that "are characterized by the ease with which they can be disaggregated and dispensed unit by small unit more or less in isolation from other units and from any general rule." In brief, the proponents of this theory of policy distribution argue that legislators prefer programs that allocate benefits broadly, including something for (almost) everyone as opposed to programs that concentrate benefits in a few select districts due to their uncertainty (which of the many possible winning coalitions will form) and their risk-aversive behavior (fear of not being part of the winning coalition).

Studies of the distribution of government benefits in such traditional pork barrel policy domains as rivers and harbors (Ferejohn 1974), defense contracting

(Goss 1972), and taxes (Manley 1970) lend empirical support to this conception of policy distribution. Studies of federal grant programs have also reached conclusions consistent with this interpretation of distributive politics (Arnold 1979; Friedland and Wong 1983; Holcombe and Zardkoohi 1981; Plott 1968; Rundquist and Ferejohn 1975; Strom 1975), although other studies have failed to confirm the saliency of politics in explaining federal aid allocations (Anagnoson 1980; Chernick 1979; Gist and Hill 1984; Rundquist 1978; Rundquist and Griffith 1976).

The mixed findings from the empirical studies in this area reflect the different structural characteristics of government programs. First, Congress does not always make explicit distributional decisions. Although decisions such as the location of military bases, federal office buildings, and water projects are made by Congress, for many programs Congress merely agrees on an overall program authorization and delegates the actual allocational decisions to the appropriate government bureaus. In some instances Congress may give a bureau guidance in terms of such things as program eligibility and maximum and minimum grant levels. However, the important point to emphasize is that much of the decision making concerning the distribution of government spending is made by bureaucrats, not members of Congress. In addition, government programs evolve over time and therefore may not always be universalistic. For instance, David Stockman (1975) argues that social programs begin as narrowly defined programs and gradually expand as subsequent reauthorization bills broaden eligibility to include more and more districts among the beneficiaries of federal programs. Robert Stein (1985) advances a similar argument concerning housing and community development programs. Thus if programs do evolve over time, one would expect the distributional patterns and their determinants to reflect these changes.

Bureaucratic Politics

Arnold's (1979, 1981a, 1981b) research adds an important dimension to our understanding of the politics of policy distribution: the exercise of influence is a two-way street between members of Congress and bureaucrats. Not only do legislators seek to influence bureaucrats in the allocational decisions they make, but bureaucrats also seek to influence legislative decision making—for example, the way senators and representatives vote on key issues affecting agency programs. According to Arnold (1979, 36), the distribution of government benefits can best be understood by examining the allocational strategies that bureaucracies pursue to maximize the legislative coalitions for their programs: "Bureaucrats allocate expenditures both in gratitude for past support and in hopes of future congressional support; and congressmen support agencies both because they owe them for past allocations and because they desire future allocations."

Yet Arnold's work, particularly his examination of the Model Cities and Water and Sewer programs, also presents a limited view of the dynamics of policy distribution. Arnold's (1979) study of the Model Cities program is based on the argument that Washington bureaucrats in the Department of Housing and Urban Development strategically awarded model cities designations among key congressional districts in order to enlarge the program's constituency within Congress. Arnold found that committee representation and votes in support of funding for model cities were the principal determinants of whether or not cities from a representative's district were selected for participation in the Model Cities program.

What Arnold's analysis fails to account for, however, are the contextual characteristics of the individual cities that applied for funding. When one considers such factors as population size, previous program experience, and the level of city need, the political effects of the model cities selection process are attenuated.[2] Cities that were selected for the Model Cities program tended to be large, relatively distressed cities with active urban renewal programs. Indeed, only 7 of the 105 cities included in this study that were designated as model cities participants were not active participants in the Urban Renewal program at the time of their selection. Studies such as Arnold's either consider the contextual characteristics of recipient governments irrelevant in distributional decisionmaking or assume that the demand for federal aid and the capacity of local governments to apply for aid are uniform across all jurisdictions. Yet as Aiken and Alford (1974) have shown, not all cities wish to "innovate," or participate in federal programs. Other studies have found that the capacity of cities to prepare grant applications and secure federal funding varies substantially across communities (Chernick 1979; Hawkins and Stein 1977; Hawley 1963; Saltzstein 1977; Stein 1979).

In addition, many studies that examine bureau-legislative interactions and the distribution of government benefits present an oversimplified view of bureau decisionmaking, one that portrays more departmental unity than may actually exist. Many analysts (e.g., Cingranelli 1983, 1984; Dye and Hurley 1978; Gottschalk 1981; Holcombe and Zardkoohi 1981; Johnston 1978; Lowery, Brunn, and Webster 1986; Owens and Wade 1984; Ray 1980a, 1980b; Ritt 1976; Stein 1981) use total federal expenditures or total departmental expenditures as the dependent variable for analyzing the distribution of government benefits. While it may be important to get the big picture, aggregation at such a broad level most likely masks several relationships since most departments administer a number of grant programs, each with its own distributive biases. For example, the Department of Housing and Urban Development (HUD) administers housing programs (primarily allocated to low-income persons) and community development programs (generally allocated to places). The distributional pattern under each type of program is quite distinct. To combine allocations among all the various types of grant programs into departmental, functional, or total domestic allocations only serves to disguise and confound any relationships that might exist between political representation and the distributional patterns observed. In short, there are no departmental or total governmental strategies for allocating resources among jurisdictions. To expect to find any evidence of political influence at the committee level over aggregate allocations does not coincide with our understanding of federal expenditure politics. As Anton, Cawley, and Kramer (1980, 18) noted, "A considerable literature on national budgeting, furthermore, makes clear that federal resource allocation takes place through a fragmented process of program-by-program calculation, involving individual program managers, agency and departmental leaders, O.M.B. officials, relevant congressional committees, and often supportive clientele groups. . . . The politics of federal spending is very much a politics of individual federal programs."

Grantsmanship

Another cut at the politics of policy distribution focuses on the role of recipient jurisdictions. Here the political linkages are portrayed as vertical (federal-local) as opposed to horizontal (Congress-bureaucracy) and follow the functional lines of authority between federal, state, and local officials brought about by the proliferation of categorical grant programs in the 1960s. However, grantsmanship models of the distribution of federal funds may merely represent another image of the Congress-bureaucracy linkage in that those communities that receive a disproportionate share of federal funds may rely upon their representatives and senators to do their bidding for them. In contrast, however, there may be political linkages outside the legislature that account for the distributional patterns, particularly linkages between presidential administrations and influential mayors and governors who can follow through on their promise to deliver the vote on election day or who have done so in the recent past.

There may also be reasons beyond political influence—or political influence operationalized from an electoral perspective—that may account for selected cities receiving a disproportionate share of grant funds. Some communities may receive larger allocations because their needs are greater. Other communities may have more experience and familiarity with federal programs, which may or may not translate into more meritorious grant applications. For example, communities with greater experience in low-rent public housing programs were more likely to receive allocations in the initial rounds of urban renewal funding than were those cities with little or no prior federal program experience. Similarly, those cities with prior urban renewal experience were more likely to receive designation under the Model Cities program than were cities without previous HUD experience. Some cities may have a greater capacity to secure grant funds. For instance, they may have a specific local agency charged with following grant developments or a Washington lobbyist or consulting firm to assist them with grant applications. Studies indicate that lobbyists do make a difference. Peliserro and England (1980) found that cities with a grant or liaison person in Washington were more successful in securing federal grants than were cities without such lobbying assistance. Cingranelli (1984) reported similar results for state governments. Finally, as Stone and Sanders recently noted in their analysis of New Haven's experience with the Urban Renewal program, some cities simply have greater demand for federal aid than other cities. "The distribution of renewal funds came as a result of local applications. Washington could not and did not 'tap' particular cities in the absence of formal applications; but it did respond to the pipeline of local requests in a first come, first served manner. Local applications were therefore not evaluated competitively. Whoever met the basic requirements for programs and projects got money" (1987, 172).

Summary

The major flaw in the models of distributive politics outlined above is an overemphasis on a centralized, top-down view of policy distribution. While the subject of inquiry is the distribution of federal expenditures, most studies have failed to incorporate federal features into their conceptualizations of the dynamics of policy distribution; that is, by focusing on the role of presidents, legislators, and bureaucrats, scholars have ignored a class of participants that have become key actors in the distribution of federal aid—the recipient jurisdictions. Indeed, for many federal programs, state and local governments are the key decision makers in regard to the distribution of federal aid, for they are the ones who decide whether or not to apply for aid, how much to ask for, and how often they will seek assistance. For a number of years many communities received little or no federal aid—not because they lacked influential legislators but because they didn't want any federal aid. This was especially evident among southern and suburban communities during the 1960s, who feared that acceptance of federal aid under the Urban Renewal program would require them to take actions to address low- and moderate-income housing needs within their jurisdictions.

In summary, the determinants of the

distribution of federal expenditures are multifaceted, complex, and dynamic. While many refer to the "federal aid system," there really is no system but rather a collection of numerous programs, each with its own unique distributional characteristics. If we are to understand more fully the relationships between politics and federal grant allocations, we need to design studies that incorporate all of the key participants and allow one to assess these relationships at different periods during the life cycles of federal aid programs.

Research Design

The conceptualization of policy distribution used in this study emphasizes the role of federal and local participants, the importance of federal programs as mechanisms for allocating government benefits, and the dynamics of program allocations and their determinants. The model tested in this study posits that the distribution of federal funds for community and economic development is a function of three factors: the political influence of city representatives and senators, the level of need within communities, and the demand and capacity of local governments. The research design includes both comparative and longitudinal features and examines the allocation patterns of six federal grant programs—Urban Renewal, Model Cities, Community Development Block Grants (CDBG), Urban Development Action Grants (UDAG), Title I Public Works, and Local Public Works (LPW)—administered by two federal agencies, the Department of Housing and Urban Development (HUD) and the Economic Development Administration (EDA). (See Table 1.) In its broadest sense, the study compares two different but somewhat similar sub-governments—the department of Housing and Urban Development and the Economic Development Administration and their relevant authorization and appropriations committees and participating cities.

The research design is also comparative in the sense that it includes both project grant and formula grant programs within each agency. While previous studies have compared the relative responsiveness to community needs of both project grant and formula grant programs (Copeland and Meier 1984; Hedge 1981), these comparisons have generally been aggregate ones, that is, a comparison of all—or a group of—formula grants versus project grants. Federal aid programs, however, have a variety of different objectives, eligibility criteria, and allocational systems. Some programs are intentionally designed on a "fair share" principle and distribute funds on a relatively equal basis, whereas other programs have "worst first" allocational systems that are more redistributive and tend to be more responsive to local needs. To compare aggregate formula and project grant program allocations is not the most effective way to compare the relative responsiveness of each type of grant program to local needs. For instance, there might be more variation within groups (formula grants versus project grants) than across groups. A research design that specifically focuses on formula and project grant programs within the same functional area and with similar national objectives permits a more precise comparison of the relative responsiveness to community needs of each type of grant program. The longitudinal aspects of the research design are incorporated through analysis of the distributional determinants over time.

The central focus of this study is to determine to what extent political influence, community needs, and local demand and administrative capacity affect the distribution of federal grants for community and economic development.

Political influence. To what extent does

Table 1. Major Features of Federal Community and Economic Development Programs

Characteristics	Urban Renewal 1950–74	Model Cities 1967–74	CDBG 1975–83	UDAG 1978–83	EDA Title I 1966–82	EDA LPW 1976–77
Number of Cities	310	310	383	383	383	383
Allocation Characteristics						
Total constant $ billion (1967 = 100)	9.3	1.7	7.5	.95	.45	.90
Total current $ billion	9.1	2.1	16.3	2.3	.76	1.7
Number of cities funded	252	105	383	223	199	343
Cities funded (%)	81.3	33.9	100	58.2	51.9	89.6
Applicant cities funded (%)	—	74.5	100	88.5	—	—
Number of eligible cities	—	105	383	257	330	—
Cities eligible (%)	—	33.9	100	67.1	86.2	—
Eligible cities funded (%)	—	100	100	91.1	60.3	—
Grant Characteristics						
Allocation type	categorical	categorical	formula	categorical	categorical	formula
Federal share (%)	67	80–100	100	100	50–80	100
Eligibility criteria	state-enabling legislation	HUD selection	50,000 or more population	need (unemployment, poverty, aged housing, growth lag, job lag, income growth)	need (unemployment)	need (unemployment)
Administering agency	HUD	HUD	HUD	HUD	EDA	EDA

Note: Acronyms are abbreviations for Community Development Block Grants, Urban Development Action Grants, Economic Development Administration, Local Public Works, and Housing and Urban Development.

Table 2. Relationship between Allocation Models and Federal Grant Allocations by Program

Allocation Models	Urban Renewal	Model Cities	CDBG	UDAG	EDA Title I	EDA LPW
Total program						
Local demand	—	—	P	P*	P	P
Prior experience	P*	N	P*	P*	P	P*
Need	P	N	P*	P*	P*	P*
Political influence—House	P	N	P	P*	P	N
Political influence—Senate	N	P	P	P	P*	N
Annual or biennial						
Local demand	—	—	P	P**	P**	P*
Prior experience	P**	N	P**	P**	P	B*
Need	P**	P**	P**	P**	P**	P*
Political influence—House	P**	N	N	P**	P**	N
Political influence—Senate	N**	N	P	P	P	both

Note: Classification of the direction of the relationship in the annual or biennial allocation models is based on the direction recorded in a majority of the regression models for that program. Regression statistics are available from the author. P = positive relationship, N = negative relationship.

*$p < .05$.

**$p < .05$ in a majority of the regression models.

representation on key House and Senate committees influence the allocation of federal grants? Are allocations under community and economic development programs distributed so that members of Congress with the most influence over agency programs disproportionately benefit in the allocation of funds to cities in their districts? What effect, if any, do changes in presidential administration have on the distribution of federal grants? For instance, do Democratic and Republican administrations channel federal aid to different types of communities?

Community needs. How responsive to community needs is each of the six programs investigated? Are there distinct patterns of responsiveness across agencies or programs? Are there structural features of federal programs that make some programs more responsive to local needs than others? For example, are formula grant programs more or less responsive to community needs than are project grant programs?

Local demand and administrative capacity. What effect does the demand and capacity of local governments have on the distribution of federal grants? Do cities with greater demand and administrative capacity participate more frequently and receive larger grant allocations than cities with lower levels of demand and capacity?

Determinants of Federal Grant Allocations

Multivariate regression analysis was used to estimate the effects of each of the three types of determinants on the allocation of federal grants to U.S. cities with populations of 50 thousand or more. For each program, models were estimated for two different time periods: (1) total program allocations, which cover the entire history of the program; and (2) biennial allocations, which include the two fiscal years during each congressional session.[3] Table 2 briefly summarizes for each of the six programs the direction of the relationship and statistical significance for each of the three types of determinants analyzed. The Appendix lists and defines the variables employed in the regression analysis.[4]

Political Influence

Political benefits come in many sizes and shapes. Most previous studies looked for evidence of a disproportionate share of funds allocated to committee members' districts. Political benefits can also be widely dispersed, as was the case with the CDBG and Local Public Works programs in which nearly every city received an allocation for at least one project. In such formula grant programs as these the scope of key legislative decision makers tend to expand beyond the traditional subgovernment participants to include all members of the legislative body. For instance, issues relating to the number and type of eligible jurisdictions, the share allocated each type (small cities versus large cities), and the formula elements used to calculate recipient allocations were debated on the floors of both houses of Congress (Markusen, Saxenian, and Weiss 1981; Nathan 1987). Adoption and revision of the formula allocation systems used in the General Revenue Sharing (Nathan and Adams 1977; Nathan, Manvel, and Calkins 1975) and CDBG programs (Dilger 1982; Dommel et al. 1982) are classic examples of this expansion of the forum for debate on the distributional features of federal programs. It is not surprising, therefore, that formula grant programs tend to take on universalistic characteristics.

Project grant programs, on the other hand, tend to conform more closely to the particularistic nature of political benefits. The key decision makers are generally bureaucrats and the assumption is that allocations are made strategically, with an emphasis on building and maintaining support among those legislators with the greatest influence over agency programs, that is, the representatives and senators who serve on the appropriations and substantive committees and subcommittees. There were times when programs such as Urban Renewal, Urban Development Action Grants, and EDA's Title I Public Works program demonstrated allocation patterns that were consistent with this pattern of political benefits.[5]

Yet political benefits do not appear to be constant over time. As the data illustrate, programs and their distributional patterns fluctuate dramatically from year to year. Programs that show no effect for the political variables on total program allocations do show effects in certain isolated instances. On the other hand, programs that demonstrate evidence of political effects for total program allocations do not do so consistently over time for the disaggregated analyses of the distribution of program funds.

When analyzed on an annual or biennial basis, the incidence of political effects turns out to be quite interpretable in terms of the life cycles or stages of program development. The political representation variables were most frequently found to be statistically significant during the initial years of a program (e.g., Urban Renewal, EDA Title I Public Works, UDAG), which suggests that allocations under these programs may have been strategically allocated to reward key members of the appropriations and legislative committees. Political effects were also likely to appear during those times in which program continuation was most uncertain. Such was the case for the Urban Renewal program during its latter years in the early 1970s. EDA's Title I Public Works program demonstrated a similar pattern during this same period when President Nixon sought to terminate EDA.

Presidential politics and the distribution of federal grants. A review of the history of federal community and economic development programs is consistent with the arguments raised by Piven and Cloward (1971), Mollenkopf (1983), and Friedland and Wong (1983), among others, concerning the linkages among presidential administrations, their attendant political coalitions, and preferences for different types of federal urban programs. The Urban Renewal program was signed into law by President Truman in 1949, and initial allocations heavily favored the nation's biggest cities. Model Cities was enacted during the eighty-ninth Congress with its huge Democratic majority. President Nixon dismantled many of the HUD categorical programs by introducing a "New Federalism" in which all communities received funding, including many suburban jurisdictions that had not participated in prior HUD programs. The Carter administration introduced legislation resulting in larger allocations for the neediest cities under the CDBG program and enacted a new program, UDAG, that was more traditionally focused on the community and economic development needs of the nation's most distressed cities.

Although the programmatic history of the federal government's involvement in urban affairs fits nicely with an electoral politics interpretation, examination of Table 3—which reports the share of funds allocated to three groups of cities by presi-

Table 3. Program Funds Allocated by Size, Region, and Distress by Presidential Administration (%)

Program	Big Cities[a]	Frostbelt Cities[b]	Distressed Cities[c]
Urban Renewal			
Truman, 1950–52	51.3	63.8	29.2
Eisenhower, 1953–60	50.8	68.5	33.0
Kennedy, 1961–63	37.0	70.8	49.2
Johnson, 1964–68	30.9	66.1	41.1
Nixon, 1969–74	36.6	56.2	38.6
Model Cities			
Nixon, 1969–74	56.0	62.8	30.5
Community Development Block Grants			
Ford, 1975–76	40.3	56.8	50.9
Carter, 1977–80	46.9	58.8	53.7
Reagan, 1981–84	45.5	60.1	55.2
Urban Development Action Grants			
Carter, 1978–80	35.9	64.9	54.4
Reagan, 1981–84	34.5	73.6	61.4
EDA Title I Public Works			
Johnson, 1966–68	19.7	34.6	60.3
Nixon, 1969–74	34.1	56.9	58.6
Ford, 1975–76	39.2	63.8	56.1
Carter, 1977–80	38.5	54.1	54.3
Reagan, 1981–84	33.9	52.2	63.5
EDA Local Public Works			
Ford, 1976	48.5	66.9	57.3
Carter, 1977	43.7	64.1	55.9

[a] Cities with population of 500 thousand or more.

[b] Cities within the Northeast or Midwest census regions.

[c] Cities ranking within the most distressed quartile on the composite city needs index.

dential administration: cities with populations of 500 thousand or more; cities from the Northeast and Midwest frostbelt regions; and cities ranking in the most distressed quartile as measured by the composite city needs index—suggests that presidents have been less successful in redirecting the distribution of funds under federal programs than has been previously assumed.

The data for the Urban Renewal program show that program allocations during the Eisenhower years were fairly similar to those made by the Truman administration during the initial years of the program. Indeed, urban renewal allocations actually looked more "Democratic" since a somewhat greater share of funds were allocated to frostbelt cities and the most distressed cities. During the Kennedy-Johnson years the amount of funds increased dramatically, although the share of funds allocated to big cities dropped sharply. President Nixon was relatively successful in reorienting allocation priorities during the program's latter years as the share of funds awarded to frostbelt cities declined from about 68% in the Kennedy-Johnson years to 56% during the Nixon years.

Distributional patterns under the CDBG program indicate that the Democrats were able to redirect funding priorities under the block grant program as the proportion of funds allocated to big cities, frostbelt cities, and distressed cities increased during the Carter administration, the result of legislative changes enacted in 1977. Table 3 shows, however, that these trends continued during the Reagan years. Program allocations under the UDAG program, enacted as the centerpiece of President Carter's urban policy, actually look more "Democratic" during the Reagan years than during the Carter years.

The share of funds allocated to big cities and frostbelt cities by EDA's Title I Public Works program increased substantially during the Nixon-Ford years over what it had been in the Johnson administration, a reflection, perhaps, of why President Nixon tried to eliminate the EDA. Allocation patterns under EDA's LPW program are nearly identical under both the Ford and Carter administration. One should note, however, that LPW was a Democratic program, since the legislation was enacted by a Democratic Congress over President Ford's veto.

Community Needs

The responsiveness of federal programs to urban hardship has captured the interest of scholars and practitioners for some time, and a great deal of empirical research has been conducted on this topic during the past decade. Despite the variety of different approaches and methods used to measure community needs, there is evidence of convergence toward a broader concept of urban hardship (Burchell et al. 1981). While there are important differences among cities in extent of hardship, based on the type of indicators selected, generally the same cities (for example, Buffalo, Camden, Cleveland, East St. Louis, Newark) emerge at the top of the rankings as the most distressed places.

Several studies have examined the direct effects of community need on the allocation of federal funds. While a number of these studies focused on individual federal programs (Bunce and Glickman 1980; Cho and Puryear 1980; Dilger 1983; Dommel 1980; Gist 1980; Nathan and Adams 1977; Nathan, Manvel, and Calkins 1975; U.S. Congressional Budget Office 1978; Webman 1981), others have taken a much broader perspective and have examined total departmental expenditures (Dye and Hurley 1978), spending in functional areas (U.S. Congressional Budget Office 1977) or total federal domestic outlays (Dye and Hurley 1978; Gottschalk 1981; Stein 1981; U.S. Congressional Budget Office 1977). Studies of individual program allocations generally conclude that the distribution of federal aid is related to the incidence of community need, although the strength of that relationship varies considerably by program and by the definition of need employed. Studies that examine total departmental expenditures or total domestic outlays detect little, if any, relationship between need and the amount of aid received.

At the center of much of this research lies a debate over the structural characteristics of federal programs: Are formula grant programs more effective than project grant programs in targeting federal aid to the neediest communities? Robert Stein (1981) maintains that one of the benefits of the shift to the New Federalism was that many cities that were previously nonparticipants in the federal categorical grant-in-aid system became participants through their entitlement status in the general revenue-sharing and block grant programs begun in the 1970s. Stein's analysis, based on total per capita direct federal aid allocations to municipalities with populations of 25 thousand or more, shows that one of the principal effects of the New Federalism was a greater equalization of federal aid distributions, as these newly funded cities tended to be those most in need.

A strikingly different picture emerges, however, if one confines the analysis to the distributional patterns within a specific functional area, such as community development. Enactment of the CDBG program in 1974 brought entitlement status to all central cities and all cities with populations of 50 thousand or more. Of the 103 cities with no previous urban renewal or model cities experience included in this study that attained entitlement status under the CDBG program, nearly 90% have populations between 50 thousand and 100 thousand, two-thirds are suburbs, and more than one-half rank among the best-off cities as measured by the composite city needs index. These percentages increase further if one examines the characteristics of the cities without any previous HUD categorical experience. For every measure, cities with no previous experience were substantially better off than those cities with previous HUD categorical experience (Table 4). Thus, contrary to Stein's findings for the federal aid system as a whole, the data demonstrate that the CDBG program resulted in a spreading of federal funds to many relatively well off places.

However, while CDBG resulted in a spreading of funds to many relatively well off places, it also increased the targeting of allocations to the most distressed places. Although a greater share of grant allocations under the categorical programs folded into the CDBG program were allocated to distressed cities as a group, allocations among cities within the most distressed group were not very responsive to the level of city need. This is manifest in an examination of the regression models for the Urban Renewal and Model Cities programs, which rarely report statistically significant coefficients for the need variables, as opposed to the models for the CDBG program, which consistently report a statistically significant coefficient for the need variable in the annual allocation models.

Overall, the analysis of the six federal programs included in this study indicates that the federal government's ability to target funds to the most distressed cities increased over time. Both within programs and across programs, targeting was greater in the 1970s and 1980s than in the 1950s and 1960s. More important, the effects of need on the distribution of federal aid continue to hold when one controls for such other factors as population size, political representation, and the prior program experience of city governments. The relationship between city need and total federal grant allocations was positive for every program but Model Cities. For three programs (UDAG, EDA Title I

Table 4. Difference-of-Means Test between Cities with and without Urban Renewal and Model Cities Experience, for Community Development Need Indicators

Need Indicator	Cities without Urban Renewal and Model Cities Experience	Cities with Urban Renewal and Model Cities Experience
Composite needs index, 1970[a]	-66.6	22.0**
Population change, 1960–70 (%)	50.1	16.8**
Poverty, 1969 (%)		
Families	6.5	10.1**
Persons	8.4	13.1**
Median family income, 1969	11,289	9,659**
Net change in median family income, 1959–69	4,366	3,699**
Unemployment rate, 1970	4.4	4.7*
Aged housing, 1970 (%)	21.4	45.1**
Change, retail and manufacturing employment, 1967–77 (%)	47.6	13.2**
Number of cases	103	280

*$p < .05$.
**$p < .01$.
[a] Composite index is based on the following indicators: percentage population change, 1960–70; percentage employment change, 1958–67; net change in median family income, 1959–69; unemployment rate, 1970; percentage poverty, 1969; and percentage of housing units in 1970 built before 1940. The index is standardized at zero with cities scoring less than zero relatively better off and cities scoring above zero relatively worse off.

Public Works, and EDA LPW) the regression coefficients for the need variable were also statistically significant. The regression results from the disaggregated allocation models also support the notion that the ability of federal programs to target funds on community need increased over time as the need variable was found to be statistically significant more frequently in the latter than in beginning program years. These findings, when considered in conjunction with those reported earlier for the political variables, suggest the existence of a trade-off between responsiveness to political concerns and responsiveness to community needs.

Local Demand and Prior Program Experience

Local demand for federal aid and the extent of city participation in prior categorical programs were important factors in the allocation of grants-in-aid. Measures of local demand, which included such indicators as whether or not a city had a Washington lobbyist and the number of grant applications submitted, were positively related to total program allocations for each of the four programs for which these data were available. However, only the UDAG program (the only program for which city data on the number of applications submitted were available) shows a statistically significant relationship between local demand and total program allocations. The direction of the relationship between local demand and grant allocations was also positive in the annual or biennial allocation models. In addition to the UDAG program, both EDA programs reported statistically significant coefficients in one or more biennial equations for the local demand variables.

Overall, prior experience related positively and statistically significantly to total program allocations in five of the six programs (all but Model Cities). However, as noted earlier, previous experience with the Urban Renewal program was the most important factor explaining the selection of cities to participate in the Model Cities program. In the disaggregated allocation models, the relationship between prior experience and program allocations was similar to that observed for total program allocations. Model Cities was the only program that showed a predominantly negative relationship between prior experience and funding allocations. This negative relationship suggests that the allocation priorities under the Model Cities program were quite different from those operating for the Urban Renewal program, a finding that is consistent with the case study literature on these programs (Banfield 1976; Frieden and Kaplan 1975; Haar 1975).

Conclusion

Federal programs are not static entities. The results reported in this study clearly show that grant programs evolve over time or—as some have noted—experience life cycles (Bassein and Schulman 1984; Hogwood and Peters 1982). Two findings emphasize this point. First, distributional patterns varied over time as the share of funds allocated by population size, region, and level of distress fluctuated widely from year to year. For example, in the Urban Renewal, UDAG, and EDA Title I Public Works programs, all showed allocation patterns with a greater emphasis on larger cities in the programs' early years. Second, while political effects were not uniform across time periods, a pattern does emerge regarding when political factors were prominent in distributional decisions. The political representation variables were found to be statistically significant more frequently during the initial years of a program and during those times when program continuation was seriously challenged by either the president or Congress, periods in which the nurturing of legislative coalitions are most important. These findings clearly demonstrate that efforts to analyze total program allocations or aggregate allocations over long periods of time may mask important variations in both the allocation patterns and their determinants.

The structural characteristics of grant programs appear to account for more variation in the allocation patterns observed than do agency or subgovernment characteristics. The distribution of federal funds under HUD and EDA project grant programs were more similar to each other than they were to the formula grant programs within their respective agencies. In addition, the two formula grant programs showed parallel distributional patterns although they were administered by different agencies. Thus, based on the allocational patterns reported here it appears that the structure of federal aid programs, not the substantive nature of the policy area, shapes the politics of policy distribution.

Related to the above point, political factors were more evident in the project grant programs than in the formula grant programs. However, even in the project grant programs, political influence, as measured by representation on the appropriations and substantive committees with oversight responsibilities for these programs, accounted for very little in the distribution of program funds. One should not, however, conclude that politics is unimportant in the allocation of grants for community and economic development. Many of the case studies written on these programs have shown the importance of politics in the allocation of funds to specific jurisdictions. The findings from this study, however, show no consistent support for the notion that political influence, as measured by representation on key congressional committees, predominantly characterizes the allocation patterns of any of the six programs examined. This finding is especially noteworthy in regard to EDA's Title I Public Works program, which has frequently been categorized as a typical pork barrel program. The data show, however, that despite widespread eligibility (estimates report that communities in more than 80% of U.S. counties are eligible for EDA assistance), EDA officials do a relatively good job of targeting grant allocations to the neediest places. This finding also points out the importance of distinguishing between the distribution of eligible jurisdictions and the distribution of program funds among recipient communities.

The study's findings also challenge the traditional perceptions scholars hold concerning legislative behavior in regard to the allocation of government funds. Congressmen are portrayed as pork barrelers and log rollers interested in spreading benefits as widely as possible. The findings reported in this study, however, suggest that Congress has become more interested in targeting and less interested in pork-barreling. Over the past 35 years Congress has become more involved in the design of federal programs and has shown less willingness to grant complete discretion to the bureaucracy over the allocation of federal funds. These concerns are manifest in such actions as legislatively prescribing needs-based eligibility criteria for grant programs (EDA Title I Public Works, EDA LPW, UDAG) and determining the factors used in allocating program funds (LPW, CDBG). Moreover, recent votes in both the House and the Senate concerning the reauthorization of community and economic development programs indicate that Congress is concerned about targeting. On several recent occasions the House and Senate voted to reject amendments that would terminate the UDAG program or broaden the project selection criteria to include less of an emphasis on city need. Similarly, both the House and Senate have rejected amendments to terminate EDA programs and the House recently passed EDA reauthorization legislation that included more stringent needs-based eligibility criteria. Although congressional decisions concerning federal community and economic development programs may exhibit universalistic tendencies, these seem to be more concerned with ensuring an opportunity for all communities to participate in federal programs; Congress appears quite willing to allow bureaucrats to discriminate among jurisdictions on the basis of need in awarding program funds.

The demand and capacity of local governments are important factors in the allocation of federal grants. Indeed, for many programs, there is a strong association between demand for aid and the amount of funds awarded. For example, urban renewal allocations were based on the size and scale of the local projects submitted for HUD approval. Model cities funds were used predominantly by local officials to pay the local share of federal grants received from other agencies. Thus, the more cities chose to spend and the greater their success in seeking other categorical grants, the more model cities funds they received. Data for the UDAG program show a direct relationship between the number of applications filed and the amount of funds received. These findings stress the importance of regarding local officials as key decision makers in the federal grants allocation process. In many instances the decision to apply and the amount of funding requested may be the most critical decisions affecting the distribution of federal funds.

Finally—and perhaps most importantly—the study's findings accent the need for "disaggregating the concept of government" (Rose 1983). Not only is it necessary to examine program allocations on a less-aggregated basis in terms of time, but one also needs to disaggregate political institutions. For example, the findings reported here show that city representation on key House and Senate legislative committees were just as likely to affect the allocation of grant funds in opposite directions as in a concurrent fashion. Many previous political benefits studies failed to examine the effects of Senate representation on the assumption that the House was a more distributive-based institution. Yet the Senate played an important role in shaping the structure of many of the programs examined in this study. Lastly, one must remember that policy making in the United States takes place in a federal system where state and local governments play an important role in the development and implementation of domestic public policies. For many programs, including those examined in this study, decisions made by state and local officials have more of an impact on the distributional patterns of federal programs than those made by legislators and bureaucrats. Mayors, representatives, senators, bureaucrats, and presidents are important governmental actors seeking to influence the distribution of benefits. The element that brings all these participants together is the federal grant-in-aid program.

Appendix: Operationalization of Variables Used in Multivariate Analysis of Federal Program Allocations

The variables employed in the cross-sectional regression analysis of the distribution of federal aid are here listed and defined. A more detailed listing of the variables and their sources can be obtained from the author on request.

Dependent Variable

The dependent variable in each model is the amount of federal aid (in thousands of dollars) received during the designated time period. Annual allocation data for each program were gathered for all U.S. cities with a population of at least 50 thousand from agency records and archives.

Control Variables

Population. City population based on 1950, 1960, 1970, or 1980 census. Variable was transformed by taking the natural log of population size.

Region. A dichotomous variable taking the form of one for cities from the Northeast and Midwest and zero for cities from the South and West.

Local Demand and Administrative Capacity

Prior experience—Urban Renewal. Regression model for the eighty-first Congress uses the number of low-rent public housing programs (maximum of four) a city had participated in as of October 1948. Subsequent regression models use an index of the breadth of Urban Renewal experience based on the number of component programs in which cities were participants.

Prior experience—Model Cities participant. A dichotomous variable taking the form of one for cities participating in the Model Cities program and zero for cities that did not participate.

Prior experience—HUD programs. The amount (in thousands of dollars) of a city's hold harmless entitlement that reflects the annual average amount of funds received during the period 1968–72 from the seven HUD categorical programs (including Urban Renewal and Model Cities) folded into the CDBG program.

Prior experience—EDA programs. Cumulative amount of funds (in thousands of dollars) received under the Title I Public Works program.

Prior experience—LPW round 1. Total amount of funds (in thousands of dollars) received during round 1 of the LPW program.

DC lobbyist. A dichotomous variable taking the form of one for cities with a Washington lobbying office or representative and zero otherwise.

Number of applications. Number of UDAG applications submitted by applicant city.

Community Needs

Need. The measure of city need used in this study is a composite measure of need based on the six indicators used as eligibility criteria for the UDAG program: population change, employment change, per capita income change, unemployment, poverty, and aged housing. In order to provide a comparable measure of city need throughout the study period, four separate indexes were constructed, with each incorporating datum centered around each of the decennial census years (1950, 1960, 1970, and 1980). Because information on per capita income and poverty was not available from the 1950 and 1960 censuses, available measures that appear to be close proxies (median family income and percentage of families with low incomes) were included as substitutes. The index is standardized at zero with cities scoring less than zero relatively better off and cities scoring above zero relatively worse off.

Political Influence

House influence. A composite measure of the influence of cities' members of congress based on committee and subcommittee membership ranging from zero to four based on the number of committee and subcommittee assignments. For HUD programs these included the Appropriations Committee and Subcommittee on HUD-Independent Agencies as well as the Committee on Banking, Housing, and Urban Affairs and Subcommittee on Housing and Community Development. For EDA programs they included the Committee on Appropriations and Subcommittee on Commerce, Justice, and the Judiciary as well as the Committee on Public Works and Subcommittee on Economic Development.

Senate influence. A composite measure of the influence of cities' senators ranging from zero to four and based on the number of committee and subcommittee assignments. For HUD programs these included the Appropriations Committee and Subcommittee on HUD-Independent Agencies as well as the Committee on Banking, Finance, and Urban Affairs and Subcommittee on Housing and Community Development. For EDA programs they included the Appropriations Committee and Subcommittee on Commerce, Justice, and the Judiciary as well as the Committee on Public Works and the Environment and Subcommittee on Economic Development.

Notes

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1. In addition to growth in the amount of federal grants, these included (1) the shift to more broad-based grant programs, such as general revenue sharing and block grants, which allotted greater discretion to state and local governments in the use of federal funds; (2) changes in how federal aid was distributed, as manifest in the move to formula grants as opposed to discretionary project grants; and (3)

the emergence of a direct federal-local partnership (Nathan 1978).

2. These findings are based on a logit analysis I conducted of the 156 cities with 1960 populations of 50 thousand or more that applied for Model Cities designation. One should note, however, that Arnold's analysis excludes the largest cities and that mine excludes the smallest cities, so our results may not be directly comparable. Furthermore, his analysis focuses on decisions regarding the selection of districts with participant cities, whereas mine examines decisions regarding the selection of cities. These results point out that interpretations of the dynamics of policy distribution are very sensitive to the units of analysis (congressional districts versus cities) selected for study.

3. The decision to use Congresses as the temporal unit of analysis is based on the assumption that the partisan alignment within the Congress and its relationship to the president's party set the overall political tenor for that period. Also, bureau-congressional relations tend to focus on Congresses, especially matters relating to the reauthorization of agency programs. While some may argue that presidential administrations may be the more appropriate unit of analysis, the use of Congresses still permits comparison of distributional patterns across presidential terms and has the added benefit of providing information for two points in time as opposed to one.

4. The regression statistics for each of the estimated models can be obtained from the author on request.

5. See n. 4.

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Michael J. Rich is Assistant Professor of Political Science and Public Policy, Brown University, Providence, RI 02912.

RESEARCH NOTES

CLOSENESS, EXPENDITURES, AND TURNOUT IN THE 1982 U.S. HOUSE ELECTIONS

GARY W. COX
University of California
San Diego

MICHAEL C. MUNGER
University of Texas
Austin

Students of elections have repeatedly found that the closeness of an election is modestly correlated with turnout. This may be due to a direct response of instrumentally motivated voters, but recent theoretical work casts doubt on the adequacy of this explanation. Another possibility is that elite actors respond to closeness with greater effort at mobilization. We explore the latter possibility by using FEC and state data on campaign expenditures in House, Senate, and gubernatorial races. Our results indicate that closeness has an effect at both the mass and elite levels. We also provide quantitative estimates of the effect of Senate and gubernatorial expenditure on House turnout.

Students of elections have repeatedly found that the closeness of an election is modestly correlated with turnout, whether one speaks of gubernatorial elections (Barzel and Silberberg 1973; Gray 1976; Patterson and Caldeira 1983), state legislative elections (Caldeira and Patterson 1982), congressional elections (Crain, Leavens, and Abbot 1987; Dawson and Zinser 1976; Silberman and Durden 1975), or presidential elections (Ashenfelter and Kelley 1975; Chambers and Davis 1978; Crain and Deaton 1977; Foster 1984; Kau and Rubin 1976). This pattern of evidence is consistent with two different hypotheses. First, more citizens may vote in close elections because they believe their participation has a greater chance of affecting the outcome. Alternatively, *elites* (candidates and their chief financial supporters, for example) may be more likely to mount "get out the vote" campaigns in close elections, for precisely the same reason. At present we have little empirical basis for deciding who responds to closeness: voters only, elites only, or both.

There are, however, substantial *theoretical* reasons to expect that the direct response of ordinary voters to closeness is mild at best, so that any correlation between closeness and turnout is predominantly a mediated one: closeness stimulates elite effort, elite effort stimulates turnout.[1] With Federal Election Commission data on campaign expenditures, we can investigate a specific version of the elite-level hypothesis for the 1982 U.S. House elections—that closer elections stimulate campaign expenditure, which in turn stimulates turnout.[2]

Previous literature finds a positive correlation between total campaign expenditures and turnout (Caldeira and Patterson

1982; Dawson and Zinser 1976; Patterson and Caldeira 1983; Settle and Abrams 1976). If campaign expenditure excels closeness as a proxy for elite activity and if such activity explains the correlation between closeness and turnout, controlling for expenditure should eliminate the apparent impact of closeness on turnout. The evidence is against this last prediction, however: closeness and expenditure both matter in existing studies. Indeed, according to Dawson and Zinser (1976), expenditure—not closeness—is *in*significant when both are used as regressors.

This indicates that (1) expenditure is not a good proxy for elite effort, (2) voters respond directly to the closeness of elections, or (3) the existing multivariate regression equations have been misspecified.[3] We investigate the last of these possibilities first, and then move to a consideration of the other two.

We first discuss the theoretical basis for believing that ordinary voters or elites will respond to the closeness of elections. Then we specify a model of turnout based on existing work and demonstrate that coefficient estimates can be improved by employing a better specification of electoral competition and by controlling for urbanization. We will add to existing specifications a more complete treatment of the potential impact of Senate and gubernatorial races on House turnout. And finally, we present some conclusions and directions for further research.

Closeness and Turnout

Much research into the relationship between closeness and turnout has been inspired by the seminal work of Downs (1957) and of Riker and Ordeshook (1968). Their premise is that citizens will participate in elections if and only if the expected benefits of voting exceed the costs, a formulation that appears to predict that closeness and turnout will be positively correlated. (The closer the election, the more likely that a single vote will prove decisive, the larger the instrumental value of voting, and the more likely that a typical voter will vote.) The problem with this line of reasoning is intimately related to the well-known "paradox of not voting" (Ferejohn and Fiorina 1974). Because the probability that a single vote will be decisive is negligible even in close elections, the instrumental value of voting is also negligible even in close elections. But then there is no prediction of a direct correlation between closeness and turnout. More precisely, the correlation is expected to be positive but trivial. As Schwartz asserts, "saying that closeness increases the probability of being pivotal . . . is like saying that tall men are more likely than short men to bump their heads on the moon" (1987, 118).

It has been suggested by various scholars that voters may overestimate the probability of their votes being decisive.[4] If this is so and if especially large numbers overestimate their impact in close elections, a direct correlation between closeness and turnout would again be predicted. But if we do not care to hypothesize that significant numbers of voters misperceive their chances of decisiveness, we must accept the negligibleness of instrumental motivations in mass elections.[5]

This leaves us, however, without any direct explanation of the correlation between closeness and turnout. The only remaining possibility is some form of indirect explanation: perhaps closeness causes *elite* actors to increase their activity in the electoral process, thus stimulating turnout. The particular version of this explanation considered here is that (1) closer elections stimulate campaign expenditures and (2) expenditures increase turnout. We now consider the two parts of this hypothesis in greater detail.

A close race might cause greater expenditure for any of three related reasons. First, instrumentally motivated contributors might give more in close elections (because their contributions have a greater chance of affecting the outcome), thereby allowing candidates to spend more. Second, even if no contributors are instrumental and all seek "access" or a specific favor (see Denzau and Munger 1986; Ferejohn and Noll 1985), one might still expect an increase in contributions in closer races. The candidates in such races will put forth a greater fund-raising effort and will be willing to promise more in the way of access or favors for a contribution of given size. Third, because candidates value a given amount of money more highly in closer races, they will be more willing to borrow, to use their own financial resources, and to incur debts that they may not be able to pay. All of these financial practices allow greater campaign expenditure in closer contests.

In practice, one finds that aggregate contributions do tend to be greater in closer elections. The correlation between percentage margin of victory in the 1982 House elections and total campaign receipts reported to the Federal Election Commission is −.46. We have been unable systematically to test whether borrowing and unpaid debts increase with closeness, but a cursory examination of the data is consistent with this hypothesis. Given that closeness does appear to stimulate campaign expenditure, the next question is whether expenditure affects turnout.

Specifying an Empirical Model of Turnout

Existing Work

Existing work on turnout has used a wide variety of specifications. Rather than review them all, we focus on the specifications used by Caldeira and Patterson (1982), Dawson and Zinser (1976), Patterson and Caldeira (1983), and Settle and Abrams (1976), all of whom examine turnout using both expenditure and closeness as independent variables. The general model is

$$\begin{aligned}\text{turnout} = {} & \beta_0 + \beta_1(\text{spending}) \\ & + \beta_2(\text{closeness}) \\ & + \beta_3(\text{demographics}) \\ & + \beta_4(\text{conditioning variables}) + \mu;\end{aligned}$$

that is, turnout in the *i*th race is a function of candidate spending, anticipated closeness, district demographic characteristics, and other conditions that are specific to the *i*th election. Within this general framework, there remains wide latitude for alternative variables and functional forms. As regards spending, for example, Settle and Abrams (1976) use total per capita expenditures. Dawson and Zinser (1976) use total spending (not per capita) and also include the percentage *difference* between candidate expenditures. Caldeira and Patterson (1982) use total expenditures and the square of total expenditures to determine whether spending has a diminishing marginal impact. Patterson and Caldeira (1983) use the same two variables but in per capita form. Similar differences occur in the measurement and specification of demographic and conditioning variables.

There is less diversity in the measurement of closeness. Caldeira and Patterson 1982 and Patterson and Caldeira 1983 both use the percentage margin of victory, or the winner's percentage minus the loser's. Settle and Abrams (1976) use the winner's percentage of the vote (which, given that nearly all the elections they studied were two-candidate affairs, is econometrically equivalent to using the percentage margin). Dawson and Zinser (1976) employ voter registration figures instead of vote totals but otherwise proceed as do Caldeira and Patterson (1982).

Of the various possible specifications we have chosen that of Patterson and Cal-

Table 1. Turnout Regressions

Variables	Model 1	Model 2	Model 3
Demographic			
Education	.0415 (2.09)	.044 (2.27)	.085 (4.77)
Income	$.97 \times 10^{-6}$ (.59)	$.81 \times 10^{-6}$ (.51)	$.25 \times 10^{-5}$ (1.70)
Age	-.0043 (-3.22)	-.0042 (-3.25)	-.0049 (-4.23)
Region	.091 (8.37)	.094 (8.92)	.089 (9.40)
Percentage urban	—	—	-.101 (-5.22)
Density	—	—	-4.3×10^{-5} (-4.95)
Expenditure			
Hexp	.011 (.83)	.040 (3.14)	.039 (3.50)
Hexp^2	-.0012 (-.49)	-.0053 (-2.17)	-.005 (-2.32)
Other			
Margin	-.102 (-3.14)	—	—
Closeness	—	$-.17 \times 10^{-5}$ (-3.20)	$-.16 \times 10^{-5}$ (-3.48)
Closeness^2	—	$.28 \times 10^{-10}$ (4.42)	$.22 \times 10^{-10}$ (5.12)
Senate	.007 (.72)	.0028 (.30)	.013 (1.57)
Closing	-.0017 (-3.46)	-.0013 (-2.64)	-.0011 (-2.50)
Intercept	.23 (.92)	.15 (.63)	-.26 (-1.20)
$\bar{R}^2$	.39	.42	.55
F	20.37	20.85	28.59
N	270	270	270

Note: Dependent variable is turnout; t-statistics are in parentheses. The relevant t-statistics and significance levels are 1.65, .10; 1.96, .05; 2.29, .025.

deira 1983 (PC) as a baseline of comparison for our own work. This specification is more complete than that in Caldeira and Patterson 1982 or Settle and Abrams 1976 and is easier to replicate than that in Dawson and Zinser 1976. The PC model is as follows (precise definitions of the variables can be found in the Appendix):

$$\begin{aligned} \text{turnout} = {} & \alpha_0 + \alpha_1(\text{income}) \\ & + \alpha_2(\text{age}) + \alpha_3(\text{education}) \\ & + \alpha_4(\text{Hexp}) + \alpha_5(\text{Hexp}^2) \\ & + \alpha_6(\text{margin}) + \alpha_7(\text{Senate}) \\ & + \alpha_8(\text{region}) + \alpha_9(\text{closing}) + \gamma \end{aligned}$$

We have estimated this model using data from the 1982 U.S. House elections (see Model 1 of Table 1). Only those races were used for which data on spending were available and in which there was both an incumbent (no open seats) and a challenger (no unopposed incumbents)—270 in all.[6] The results are on the whole similar to those obtained by Patterson and Caldeira (1983, Tbl. 2). We find, as Patterson and Caldeira and many others have, that the greater the proportion of total population older than 65, the higher the turnout and that turnout is significantly higher outside the Old South. Median education and median income both have the expected positive sign, though the coefficient on income is insignificant. Margin has the anticipated negative sign: turnout increases in close races. A simultaneous Senate race in the same state has a positive but insignificant impact. Finally, the number of days separating the close of registration from the election ("closing") has the impact predicted by Rosenstone and Wolfinger (1978): the easier it is to register, the greater the turnout.

The expenditure variables perform less well. Although expenditure appears to increase turnout at a decreasing rate (Hexp > 0, $\text{Hexp}^2 < 0$), neither coefficient approaches significance. Taken at face value, Model 1 of Table 1 indicates that purely atomistic individual choices based on the probability (proxied by margin) of having an impact on the election *do* affect turnout but that actions by elites (as measured by the expenditures variables) *do not.* We argue that this conclusion is the artifact of a misspecified estimating equation. We focus in particular on the use of the margin variable and on the exclusion of variables measuring the urbanization of district populations.

The Role of Electoral Margin in Determining Turnout

The most common measure of the closeness of elections is what we have labeled "margin," or the winner's percentage of the vote minus the loser's. Cox (1988) has urged two objections to this variable, one conceptual and one econometric. The conceptual objection is simply that a given percentage margin of victory may not indicate the true capacity of individuals to affect the outcome. Consider two different elections in which margin equals 33%. In the first, assume that there are 100 thousand voters, so that 33 thousand votes separate the winner from the loser. No single campaign worker or voter is likely to think that a little extra effort in the fourth precinct—much less a single vote—will swing this election. If on the other hand there are but three voters, a 33% margin implies a two-to-one tally, and a single vote is decisive.

The econometric objection to the use of percentage margin is more serious than the conceptual. Because total votes cast appear by construction in the *numerator* of turnout and in the *denominator* of margin, it is clearly possible that the observed negative correlation is spurious.[7] Cox (1988) argues that a superior specification is to use closeness and closeness^2 in place of margin (where closeness is the raw vote margin, or the number of votes for the winner minus the number of votes for the loser). The test of the "closeness-counts" hypothesis then rests on the sign—which should be negative—of the estimated coefficient on closeness.[8]

Our estimates of the PC model using the closeness variables instead of margin are given in Model 2 of Table 1. Three points should be emphasized. First, the coefficients of both closeness and closeness^2 are of the predicted sign and significance at the .001 level. Second, the overall fit of the regression, as measured by the adjusted r-squared, is improved. Finally, the performance of the expenditure variables, Hexp and Hexp^2, is markedly improved. Indeed, the estimated coefficient on Hexp is increased nearly fourfold, from .011 to .040, while the standard error is practically unchanged.[9] Given the similarity of our model to the PC model, the use of margin likely entails underestimation of the impact of spending in the PC model of gubernatorial races as well.

Omitted Variables: Controlling for Urbanization

The problems of bias associated with omitted variables are well known (see, e.g., Judge et al. 1980, 410–13). One variable currently omitted from our regression is the level of urbanization of each congressional district. This omission may be important, because urbanized districts tend to have both higher costs of campaigning (partly because everything is more expensive in the city) and lower turnout (a phenomenon that Squire, Wolfinger, and Glass [1987] attribute to the higher mobility—hence higher registration costs—of urban dwellers). These two separate influences of urbanization—higher campaign costs and higher voting costs—may have introduced a downward bias in the estimated coefficient of expenditure in Models 1 and 2. We employ two variables to measure urbanization in investigating this possibility. The first—percentage urban—equals the proportion of the district population living in places with 25 hundred or more persons. Because this variable by itself does not distinguish between "small town urban" and "big city urban," we also include a variable measuring the population density of each district.[10]

The results obtained by adding these two urbanization variables to Model 2 are displayed in Model 3 of Table 1. As can be seen, the coefficient on the expenditure variable—Hexp—is not increased as we had conjectured it might be; rather, it remains stable, and increases somewhat in significance.[11] Nonetheless, it is clear that including the urbanization variables improves the specification of the model considerably. Both density and percentage urban have estimated coefficients that are of the expected sign and significantly different from zero. The coefficients of education and income (both of which covary with the previously omitted urbanization variables) double in size, while their standard errors remain stable or decrease. Nearly every other estimated coefficient is similarly improved in precision. Finally, the adjusted r-squared increases noticeably, from .42 to .55.

The main point to emphasize about both Models 2 and 3 is that expenditures (Hexp) do appear to stimulate turnout in House races. We turn now to another possibility—that expenditures in other, simultaneous races also affect House turnout.

Getting to the Polls

Studies of turnout typically focus on a single office without fully accounting for the impact of campaigns for other offices. At most, a dummy variable is inserted in regression equations to control for the influence of a presidential or senatorial contest.

There is reason, however, to expect that specific characteristics of presidential, gubernatorial, and senatorial campaigns will affect turnout in lower offices. This can be seen by considering the case of House elections. Let t denote the number of votes cast for U.S. representative in a given district, and let e denote the number of adults over eighteen in the same district. Then t/e is the turnout rate as usually calculated. This figure, however, can usefully be expressed as the product of two slightly different turnout rates, as follows:

$$\frac{t}{e} = \frac{b}{e} \cdot \frac{t}{b} .$$

In this equation b is the number of persons who enter the voting booth. Thus, b/e is the proportion of age-eligible citizens who go to the polls. The second term, t/b, is the proportion of those in the booth who participate in the House election.

Metaphorically, t/b (or $1 - (t/b)$) measures only the last and lowest hurdle facing voters. The first hurdle is registering. The second hurdle is getting to the polls and entering the booth. The last is actually marking the ballot to signify a vote in the race for U.S. representative.

From the perspective of explaining levels of turnout this last hurdle is also the least important. Typically over 90% of those entering the booth cast a vote for a House candidate; it is the previous two hurdles that account for the vast majority of those age-eligibles who do not.[12] This point is important because the proportion of age-eligibles entering the booth (b/e) is potentially affected by a wide variety of factors. The closeness and level of expenditure in the House election may affect the decision to enter the booth. But so may the closeness and level of expenditure in other major contests, especially those for governor, senator, and president. Thus it is sensible to consider including closeness and expenditure variables for *all* major races, even when the researcher's interest focuses on House turnout. The extent to which such other races affect House turnout is a matter for empirical investigation.

In order to begin such an investigation, we have respecified Model 3 as follows:

$$\begin{aligned} \text{turnout} = {} & \alpha_0 + \alpha_1(\text{demographic variables}) + \alpha_2(\text{House expenditures}) \\ & + \alpha_3(\text{House closeness}) + \alpha_4(\text{closing}) \\ & + \text{Senate}[\beta_0 + \beta_1(\text{Senate expenditures}) + \beta_2(\text{Senate closeness})] + \text{governor}[\partial_0 \\ & + \partial_1(\text{gov expenditures}) \\ & + \partial_2(\text{gov closeness})] + \epsilon. \end{aligned}$$

The dummy variables Senate and governor take values of unity only when a Senate or gubernatorial race is held simultaneously with the House race; otherwise, both are zero. Thus, in states having neither a Senate nor a governor's race (so that Senate = governor = 0), the model is exactly as before; but in states with a Senate or gubernatorial race or both the separate impacts of these races are measured. The constant terms, Senate * β_0 and governor * ∂_0, allow us to test the proposition that the mere occurrence of these races, with associated media attention, influences turnout—independently of the closeness of the races or the level of campaign expenditures.

Four estimations are presented in Table 2, each adding additional variables to the specification in Model 3. Model 4 simply adds the governor dummy, which has virtually no effect on the size and significance of the previously estimated coefficients. The governor dummy itself is positive and marginally significant, as is the Senate dummy, lending some support to the idea that concurrent gubernatorial or Senate races do boost House turnout.

The possibility remains, of course, that gubernatorial or Senate contests boost House turnout significantly only when they entail significant expenditures. We test this idea by first adding total per capita expenditures in the Senate campaign (Sexp) and then total per capita expenditures in the gubernaorial campaign (gexp). Unfortunately, both these variables take the same value for every congressional district in a given state, because campaign expenditure data for the offices of senator and governor are not broken down by district. This limitation in the data should bias our test against finding any significant cross-election expenditure effects.

Nonetheless, both Senate expenditures and gubernatorial expenditures do show a positive and significant impact on House turnout. When Sexp is added to the equation (Model 5), there is a slight adjustment of some of the demographic variables (most notably, the estimated coefficient on income increases and becomes significant at conventional levels); but the major effect on previously included variables is that the Senate dummy is reduced to insignificance (both substantively and statistically). The estimated coefficient on Sexp is positive and significant, and the adjusted r-squared of the equation improves, from .55 to .58. Adding gexp has very similar consequences (Model 6). The governor dummy becomes insignificant. The estimated coefficient on gexp is positive and significant. The adjusted r-squared improves again (this time to .61). Little else is changed.

Model 6 demonstrates convincingly that the use of dummy variables to control for cross-election effects on turnout is inadequate. An F-test comparing Model 6 (dummies and expenditure variables) to Model 4 (dummies only) rejects the null

Table 2. Turnout Models with House, Senate, and Gubernatorial Spending Figures

Variables	Model 4	Model 5	Model 6	Model 7
Demographic				
Education	.082 (4.63)	.074 (4.20)	.075 (4.46)	.076 (4.51)
Income	$.25 \times 10^{-5}$ (1.71)	$.35 \times 10^{-5}$ (2.41)	$.34 \times 10^{-5}$ (2.45)	$.33 \times 10^{-5}$ (2.39)
Age	-.0049 (-4.21)	-.0052 (-4.55)	-.0065 (-5.70)	-.0064 (-5.74)
Region	.091 (9.54)	.086 (9.22)	.081 (9.03)	.082 (8.90)
Percentage urban	-.104 (-5.09)	-.11 (-5.43)	-.101 (-5.24)	-.10 (-5.46)
Density	$-.43 \times 10^{-5}$ (-5.27)	$-.39 \times 10^{-5}$ (-4.85)	$-.43 \times 10^{-5}$ (-5.45)	$-.42 \times 10^{-5}$ (-5.35)
Expenditure				
Hexp	.038 (3.38)	.036 (3.28)	.031 (2.94)	.032 (3.01)
Sexp	—	.015 (3.77)	.016 (4.03)	.013 (3.14)
Gexp	—	—	.011 (4.82)	.011 (4.62)
Hexp^2	-.0048 (-2.23)	-.0046 (-2.20)	-.0040 (-1.99)	-.0014 (-2.08)
Other				
Closeness	$-.17 \times 10^{-5}$ (-3.60)	$-.14 \times 10^{-5}$ (-3.02)	$-.15 \times 10^{-5}$ (-3.48)	$-.14 \times 10^{-5}$ (-3.26)
Closeness^2	$.22 \times 10^{-12}$ (5.18)	$.19 \times 10^{-12}$ (4.43)	$.19 \times 10^{-12}$ (4.63)	$.19 \times 10^{-10}$ (4.49)
Senate	.014 (1.65)	-.002 (-.256)	-.003 (-.31)	-.017 (-1.35)
Governor	.011 (1.52)	.014 (1.93)	-.0059 (-.73)	.0097 (.97)
Closing	-.001 (-2.34)	-.001 (-2.17)	-.001 (-2.51)	-.0014 (-3.17)
Senate margin	—	—	—	-.00085 (-2.21)
Governor margin	—	—	—	.033 (.857)
Intercept	-.24 (-1.105)	-.13 (-.60)	-.019 (-.095)	-.049 (-.19)
$\bar{R}^2$	.55	.58	.61	.61
F	26.70	27.09	29.03	26.23
N	270	270	270	270

Note: Dependent variable is turnout; t-statistics are in parentheses.

hypothesis that the coefficients of gexp and Sexp are both zero. Including these variables clearly improves the specification of the model.[13]

Model 6 also demonstrates that both expenditure levels and closeness have independent impacts on turnout. An increase of $1 in spending per capita by House candidates increases turnout by 3.1%. A thousand-vote increase in raw vote margin decreases turnout by .15%. It is difficult to compare the magnitude of these coefficients, since the units of measurement differ. In terms of beta weights (a comparison that of course has its own problems; see Hanushek and Jackson 1977, 78), closeness has a larger impact. A one-sample standard deviation increase in closeness leads to a .45-sample standard deviation increase in turnout; the corresponding figure for Hexp is .33.

It is interesting to note that at least in our sample both Senate and gubernatorial expenditures are less effective in stimulating House turnout than are House expenditures: the estimated coefficient of Hexp is .031, roughly twice that of Sexp (.016) and three times that of gexp (.011). This difference may in part be artificial—due to our inability to disaggregate Senate and gubernatorial spending by district. It may also be due to the fact that House spending should increase both *b/e* (the proportion of the electorate going to the polls) and *t/b* (the proportion of those getting to the polls who participate in the House election), whereas Senate and gubernatorial spending should increase only the former. Although the similar magnitude and significance of the Sexp and gexp coefficients suggests that the marginal impact of Senate and gubernatorial campaign spending is similar, an F-test rejects the null hypothesis that the two coefficients are equal at the .01 level.[14]

Finally, we consider the possibility that the closeness of other races affects House turnout, separate from expenditures on these simultaneous campaigns. In Model 7, margin variables for both Senate and gubernatorial races are included. The use of the percentage difference (margin) rather than the numerical difference (closeness) is dictated by two considerations.[15] First, since the potential electorate in the simultaneous races is the entire state population rather than the district population, the econometric objections to the percentage variable do not apply. Second, in electorates of varying sizes, the percentage margin is more appropriate as a measure of closeness because it controls for the size of the electorate. This consideration was unimportant for House districts, since all are more or less the same size, but it may affect results estimating the impact of competitiveness in Senate or gubernatorial races.

As it turns out, our results indicate that more competitive Senate races *do* increase House turnout in the same state but that more competitive gubernatorial races *do not*, once expenditures in both races are controlled. The coefficient on Senate margin indicates that the average effect of going from a 51%-49% Senate race to a 56%-44% one was to decrease House turnout in the same state by .8%. The coefficient on governor margin was of the wrong sign, and insignificant.

One possible explanation for the contrast between Senate and gubernatorial effects is as follows. Both Senate and gubernatorial *expenditure* have an impact on House turnout because some expenditure goes toward get-out-the-vote efforts. These efforts get people to the polls more or less independently of their awareness of issues. And once in the booth, a substantial fraction of these people also vote in the House election. Smaller Senate margins increase House turnout in the same state because instrumentally motivated voters turn out in greater numbers. Once in the booth, these voters—who are more likely to care about some federal issue—also vote in the House election. Smaller gubernatorial margins have no effect because they stimulate participation by those interested in state issues, who then roll off (do not vote in the House election). Another possibility is that Senate and gubernatorial closeness (once expenditures are controlled) have little impact on people's going to the polls (*b/e*), but do affect roll-off, with gubernatorial closeness affecting only roll-off in state offices and Senate closeness affecting roll-off in federal offices. This hypothesis is falsifiable and could be tested, given information on the number of ballots cast in each election.[16]

Conclusion

To what extent are the turnout decisions of ordinary citizens instrumentally motivated? Are instrumental calculations regarding turnout confined to those with bigger stakes and more ample resources? Most current research, both in rational choice models and elsewhere, argues against any large instrumental component in the decisions of ordinary citizens. Nonetheless, a fairly substantial body of empirical literature, using aggregate data, finds a persistent—albeit modest—correlation between turnout and the closeness of elections, suggesting that instrumental calculations are taking place at some level.

We began this research believing that the closeness-turnout correlation was real despite some shortcomings in previous research but also that the correlation probably did not arise primarily in the reactions of the mass electorate. Rather, it seemed more plausible that closeness stimulated elite activity, thereby stimulating turnout, much as V. O. Key argued long ago in *Southern Politics* (1949). Our particular approach to exploring and testing this hypothesis is to use campaign expenditures as a measure of elite activities.

Using campaign finance data from the Federal Election Commission and the states, we have examined the impact of expenditures on turnout in the 1982 U.S. House elections. The expenditure data include not just spending by candidates in each House election but also spending by candidates in concurrent Senate and gubernatorial elections. Senate or gubernatorial expenditures may get people to the polls. Once there, they are likely also to participate in the House race.

Our results indicate that the correlation between closeness and turnout is due in part to an elite-level response rather than to a mass electoral response. Closeness clearly stimulates House expenditures and House expenditures do boost turnout. However, even controlling for House expenditures, raw vote margins still have an independent impact.[17] Even if the omitted elite activities (in-kind contributions, volunteer mobilization, etc.) were important determinants of turnout, such activities are likely to covary more with expenditures than with closeness. Thus, the significant coefficient on closeness implies at least some *direct* response on the part of ordinary citizens to the closeness of elections. We have incrementally adjusted our prior beliefs accordingly. Whether this influence results from calculations by "subelectorates" (Schwartz 1987) or some other path remains to be seen.

The work reported here also demonstrates that expenditure levels in other races (specifically, Senate and gubernatorial races) affect House turnout. Our results are not surprising, but represent the first quantitative estimates of cross-election effects. In the 1982 congressional elections, an extra dollar in per capita spending by Senate and gubernatorial candidates increased *House* turnout by 1.6% and 1.1%, respectively. It remains to determine whether these figures remain stable from year to year and to estimate the impact of presidential expenditure.

Another topic for future research is the stage at which closeness counts. One interpretation of our results suggests that closeness may not be very important in getting people to the polls. It appears that one or more high expenditure races (of several concurrent contests) bring voters out, and closeness exerts its predominant effect only on participation inside the booth (i.e., roll-off).

Data Appendix: Descriptions and Sources

The data used in the analysis came from the 1982 federal elections plus simultaneous gubernatorial races. Technically, 435 House races were available, along with 33 Senate and 32 governors' contests. All the Senate and gubernatorial data were employed in the analysis, but only 270 House races were appropriate for our purposes; the remaining 165 districts were deleted from the data set.

Four criteria were used to determine whether to retain an observation:

1. Open seats (i.e., seats with no incumbent) were deleted. We expect competition for such seats to be of a fundamentally different nature and beyond the scope of the present work.

2. Unopposed incumbents were deleted. "Unopposed" incumbents are those who had no challengers or whose challenger spent no money and received less than 5% of the vote. For such seats, no "election" really took place, and almost all votes cast were the result of turnout for other races.
3. Elections for which campaign contribution records were incomplete or unavailable were deleted. If no spending reports were made to the FEC by either the incumbent or the challenger, the observation was not used because of its potentially misleading impact on the expenditure coefficients.
4. Elections in which both candidates spent less than one thousand dollars were treated as being effectively unopposed and were also deleted. Because the FEC does not record expenditures totaling less than one thousand dollars, we really do not know the expenditure levels in these races in any event (except that they all lie somewhere between zero and two thousand dollars).

The breakdown of observations was as follows:

435 House races
–57 open seats
–27 unopposed
– 1 incomplete spending data
–80 $1000 or less spending
270 total observations

A description of the actual variables used and their sources is presented in Table A-1.

Table A-1. Variable Descriptions and Sources

Variable Name	Description	Data Mean	 Minimum	 Maximum
Turnout	number of ballots cast in 1982 House race divided by total district voting age population[a]	.426	.148	.675
Margin	votes for winner minus votes for loser, divided by total votes[b]	.256	.006	.695
Senate margin	votes for Senate winner minus votes for loser, divided by total[b]	.119	.003	.446
Governor margin	votes for governor winner minus votes for loser, divided by total[b]	.007	.005	.512
Closeness	votes for winner minus votes for loser[b]	39,390	1,197	122,700
Hexp	total expenditures in House race (incumbent plus challenger) divided by district voting age population[c]	$1.2360	.0958	6.3900
Sexp	total expenditures in Senate race divided by state voting age population[c,d]	$.732	.001	6.938
Gexp	total expenditures in gubernatorial race divided by state voting age population[e]	$1.12	.51	22.55

TABLE A-1 (*continued*).

Variable Name	Description	Data Mean	Minimum	Maximum
Age	percentage of voting age population younger than 65[f]	84.5	69.3	95.7
Region	dummy variable equal to zero if district is in one of the 12 "confederate states," one otherwise	.80	.00	1.00
Income	median family income[f]	$20,531	11,578	33,404
Education	median years of school completed by voting age population[f]	12.5	11.5	13.8
Governor	dummy equal to one if there was a simultaneous gubernatorial election in the state, zero otherwise[b]	.63	.00	1.00
Senate	dummy equal to one if there was a simultaneous Senate election in the state, zero otherwise[b]	.72	.00	1.00
Closing	the number of days between the close of registration and the election, according to state election laws[g]	24.89	.00	50.00
Percentage urban	percentage of the total population that is "urban" or lives in a place of population ≥ 2,500 (U.S. census definition)[f,h]	.7200	.1889	1.0000
Density	population density, or voting age population divided by the area of the district in square miles[f]	1,381.00	.47	63,485.00

[a] Ballot data from *Congressional Quarterly Weekly* (various issues). District voting age population from U.S. Bureau of the Census, *Congressional District Data Book* (Washington: GPO, 1982).

[b] *Congressional Quarterly Weekly.*

[c] Federal Election Commission, *FEC Reports on Financial Activity 1981–1982* (Washington: FEC, 1983).

[d] State voting age population from J. L. Gardner and L. E. Purcell, eds., *The Book of the States 1982–1983* (Lexington, KY: Council of State Governments, 1982).

[e] Thad L. Beyle, "The Cost of Becoming Governor," *State Government* 56(1983):74–84.

[f] U.S. Bureau of the Census, *Congressional District Data Book* (Washington: GPO, 1982).

[g] J. L. Gardner and L. E. Purcell, eds., *The Book of the States 1982–1983* (Lexington, KY: Council of State Governments, 1982).

[h] U.S. Bureau of the Census, *1980 Census of Population and Housing,* pts. 1–52 of *Congressional Districts of the 1980s* (Washington: GPO, 1980).

Notes

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1. Eligible citizens personally contacted by party workers are more likely to vote both in local elections (Bochel and Denver 1971; Eldersveld 1956; Rossi and Cutright 1961) and in national elections (Kramer 1970–71). Kramer estimates that among people contacted who would not otherwise have voted, between one- and two-thirds were prompted to vote.

2. Not all campaign expenditure increases turnout, of course. Many types of expenditure may be neutral or nearly so with respect to turnout (e.g., efforts of persuasion targeted at areas or groups whose turnout is already high). Some may even depress turnout (see Cox and Kousser 1981). Because the FEC reports campaign expenditure by category, one can only ask whether, on balance, expenditures increase or decrease turnout.

3. We take a fourth possibility—that elites do not influence turnout—as having been rejected by the existing literature. We also note but do not explore the possibility that Hinich (1981) advances—that voting is itself an act of contribution at the atomistic level.

4. See, e.g., Riker and Ordeshook 1968 and Moe 1980, both of which point out that candidates have an incentive to mislead their followers on this score. Further, psychological research indicates that people do often make errors when dealing with small probabilities (Fischhoff and Goitein 1984; Kahneman and Tversky 1973, 1979; Lichtenstein and Slovic 1971).

5. Another approach to restrengthening the theoretical importance of instrumental motivations is Ledyard's (1984). Palfrey and Rosenthal (1985) show that this approach fails.

6. When races without challengers *were* included, significance of the demographic variables improved, implying that poor, uneducated districts are less likely to have effective challengers, as one might expect. See the Appendix for further discussion of the criteria used to select data.

7. Turnout = total votes cast/citizens old enough to vote; margin = (votes for victor − votes for loser)/total votes cast.

8. The inclusion of closeness^2 controls for the fact that beyond a certain point raw vote margin can increase only if total turnout also increases. It is expected that closeness^2 will have a positive coefficient.

9. Beyond the three differences noted in the text, Models 1 and 2 are quite similar. The demographic coefficients are virtually unchanged in size and significance, and the conditioning variables (Senate and closing) only slightly reduced.

10. The correlatison between density and percent urban is .327—positive but not so high as to pose any problem with multicollinearity. For a useful discussion of the media factors that influence public opinion see Page, Shapiro, and Dempsey 1987.

11. It is possible, however, to derive the predicted effect of urbanization on expenditures: if the sample is confined to districts that are *both* (1) more than median percentage urban and (2) more than median population density, the coefficient on Hexp is significantly reduced in size (from .039 to .025).

12. In fact, over the past five presidential elections, the ratio of total House vote to total presidential vote exceeds 90%, with individual district ratios near 100% not at all uncommon (data from various issues of *Congressional Quarterly Weekly*).

13. Inclusion of the square of Senate and of gubernatorial expenditure did not reveal any diminishing returns.

14. The value of F(1,254) is 23.24. If one performs the same F-test in Model 7, however, the difference between the coefficients on Sexp (.013) and gexp (.011) is not significant.

15. We are grateful to Gary Jacobson for making this suggestion.

16. Crain, Leavens, and Abbot (1987) provide some support for the "closeness matters in the booth" perspective, but they use only ratios of House-to-Senate turnout and exclude expenditures as regressors.

17. One possibility is that were we able to measure other forms of elite activity (volunteer manhours of activists? unrecorded in-kind contributions?) accurately and enter them separately in the regression, then, finally, the coefficient on closeness would be insignificant.

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Closeness, Expenditures, and Turnout

Gary W. Cox is Associate Professor of Political Science, University of California at San Diego, La Jolla, CA 92093.

Michael C. Munger is Assistant Professor of Government, University of Texas at Austin, Austin, TX 78712-1087.

PIETY, JUSTICE, AND THE NECESSITIES OF WAR: THUCYDIDES' DELIAN DEBATE

CLIFFORD ORWIN
University of Toronto

Political philosophy or science first emerged in response to certain contradictions in political life that thoughtful citizens could not but face. Nowhere is this process better portrayed than in Thucydides, who of all great students of politics remains closest to the perspective of practice, at the same time showing how practice points us toward a place of critical distance from politics. The Greek political world, like all prescientific worlds, acknowledged certain gods who, as rulers, made demands on humankind. But because these were neither the only, nor in practice the most insistent, demands made on humankind, the question necessarily arose as to the gods' status in the event of conflict—as to the relative necessity of these divine demands. From this followed the further question—crucial for the emergence of political science or philosophy—as to whether the political world was ruled indeed by the gods or by necessity.

Of all writers on politics, none stays closer than Thucydides to the world of citizens and statesmen—a world of urgent particulars, where every context is partisan and every moment decisive, where speeches flow and blood along with them. He is not a political scientist and perhaps not even a political philosopher. Yet he is of interest to both, for more than anyone else he shows how the study of politics first arose out of its practice or how precisely the most political perspectives imply, on reflection, a critical distance from political life. I treat an episode quite lacking in charm, one in fact permeated by the odor of death and by meanness in the face of death; for I wish to view people trying to come to grips with an extreme situation.

In the eighth year of the Peloponnesian War, the Athenians invaded neighboring Boeotia (Thucydides *Peloponnesian War* 4.76-77 [1963-72]. Having occupied, fortified, and otherwise desecrated the shrine of Apollo at Delion (Latin, *Delium*), they suffered a decisive defeat in battle, losing the conquered territory except for the sliver on which the sanctuary stood (4.89-96). Seeking to recover under truce the bodies of their fallen—a formal supplication of the utmost solemnity that is rebuffed nowhere else in Thucydides[1]—the Athenians were advised by the Boeotians that they need not seek the bodies until they had evacuated the temple. After exchanging vituperations the two sides parted with neither having gained its object (4.97-99). The Boeotians now besieged the Athenian garrison and succeeded in driving it from the temple. They then acquiesced in the recovery of the bodies, fully 17 days after the original battle (4.100-101).

Seventeen days is a long time for

corpses in the summer sun, and most critics have followed the Athenians and George Grote (1907, 307–09) in blaming the Boeotians for flouting sacred law in the hopes of extorting an unearned victory. So, for example, Gomme for whom "Thucydides' insertion of this long dispute, his insistence on this argument of words, was due to his feeling that the Boeotian refusal to allow the Athenians to collect their dead was another evil resulting from the war . . . , an abandonment of one of the recognized, and humane, usages of Greece" (1956, 571).

What Thucydides' characters dispute, however, is not the "inhumanity" of the refusal but whether it and the behavior of the Athenians that provoked it are offenses against the law of man that forbids dishonoring the gods. Can indignation at the Boeotians' "inhumanity" really explain Thucydides' decision to report with such care this wrangling over points of sacred law? In the end even Gomme seems to doubt it. He mutters that "Thucydides is curiously interested in this sophistical stuff" (p. 569; cf. Strauss 1964, 208, n. 70). What I find curious, however, is not Thucydides' interest in this episode but the critics' lack of it—so far as I know, mine is the first schematic treatment in any language[2]—for the parties (particularly the Athenians) address two questions fundamental not only for Thucydides but for the emergence of political philosophy: the relation of justice to piety and of both to necessity.

The complaint of the Boeotians—as with all the speeches on this occasion, Thucydides reports it in indirect discourse—is

> that the Athenians had not acted rightly in transgressing the usages of the Hellenes, for it was established practice for them all, when invading each other's territories, to refrain from the sanctuaries therein. The Athenians, however, had fortified Delion and were now making themselves at home there, acting in every respect as men do on unconsecrated ground, even drawing for ordinary use the water that they themselves never touched except for use in the sacred rites. Wherefore the Boeotians, on behalf of the god and themselves, invoking the local divinities and Apollo, served *them* notice to take up their [dead] upon evacuating the temple. (4.97)

To this the Athenians reply by herald

> (1) . . . that they had not wronged the temple, nor would they willingly harm it. That was not why they had entered it, but rather that from it they might defend themselves against those who were wronging them. (2) As for the law of the Hellenes, it was that to whoever wielded power over a country, be it more or less extensive, to them the temples also always belonged, to be tended, to the best of their ability, in the ways already customary.[3] (3) After all, the Boeotians, along with most others who had taken over a country by force and expelled the original inhabitants, had begun by attacking as belonging to *others temples that they now possessed as their* own. (4) And as for [the Athenians], had they succeeded in establishing their power over more of the country of the Boeotians, so matters would have stood for them. Even as it was, moreover, they held that part of it in which they stood as their own, and would not of their own will depart from it. (5) As for the water, finally, they had disturbed it under duress [or "compulsion," *ananke*] which they had not wantonly incurred, having been compelled to use the water in defending themselves against those who had first invaded their land. (6) Besides which, anything done under stress of war and danger might reasonably claim some indulgence even in the eyes of the god. For the altars were a refuge from the consequences of involuntary offenses,[4] and *transgression* was a term reserved for those under no compulsion to be bad, not for those whom misfortune drove to dare. (7) And more impious by far were those who demanded that temples be exchanged for bodies than they who refused to relinquish temples in return for what was properly theirs already. (8) What they demanded to hear from [the Boeotians], loud and clear, was not "to depart from Boeotian territory"—for where they stood was [the Boeotians'] no longer, themselves having acquired it by the spear—but [for the Athenians] to take up their bodies under truce in accordance with ancestral ways. (4.98)

The Athenians do not deny that their actions were prima facie impieties.[5] From the outset their appeal is to the extenuating circumstances. They begin conventionally enough by discriminating between wronging and harming, invoking the respectable distinction between just and unjust wars (98.1). They suggest that nothing undertaken in a just cause could rightly be blamed as impious. They proceed, however, to a much bolder defense of their seizure of the temple (98.2-4). In expounding a right allegedly conferred by conquest, they no longer bother to distinguish between just and unjust conquest. Their point is rather that conquest as such (i.e., conquest insofar as it succeeds) confers on the conqueror legitimate possession of both lands and shrines – or how did the Boeotians come to be rightful masters of Boeotia, to which they enjoy no claim save that of having driven out their predecessors? That the Boeotians are in fact the rightful masters of that country the Athenians can afford to concede, even as they try to dislodge them from it, precisely because possession is nine-tenths of the law. Since whoever possesses does so by right, one need only dispossess him in order to possess by right oneself – pending the appearance of the next dispossessor. The tiny sliver of Boeotia that Athens still holds after defeat in the battle, however recently snatched and however soon to be retaken, it possesses by the very same title—force that conveys right—by which the Boeotians possess the rest of the region. This means, of course, that right simply collapses as an alternative to, or limit on, force.

As for the gods, these acquiesce in conquest at least as readily as humans do. Such, at least, is implied by Hellenic custom itself. The conquest once effected, life goes on, in sacred matters as in others. The subjection, expulsion, or extirpation of one people by another – what are these to the gods? The sacrificial fire is rekindled, the cult of the god resumes. The honors formerly offered by the vanquished are now the responsibility of the victors. The Athenians here develop an unwitting suggestion of the Boeotians themselves. These presented impiety as unjust but not the rest of injustice as impious: the invasion of Boeotia they no doubt regarded as unjust, but it was only the seizure of the temple that they presented as *impious.*[6] The Athenians then need only point out that temples—like oaks and rocks—come with the (conquered) territory. If it is not impiety to occupy the territory, neither can it be impious to occupy the temples; and if (as here) a shrine happens to be the first acreage seized, no offense to the gods is either intended or taken.

The argument so far, while addressing the seizure of the temple, does not address its desecration. The Athenians therefore go on to argue that the gods might reasonably take not only conquest but even what would normally be impiety in stride, accepting here the plea of *necessity* (98.5-6). In support of this claim the Athenians again adduce a fundamental practice of Hellenic piety, asylum at the altars of the gods for involuntary offenders. (The typical case was the accidental homicide.) By applying this usage to their own case, however, the Athenians notably extend it. I find in Greek literature no case of asylum at an altar for someone accused of defiling that altar.

Nor is it merely that the gods might and should accept the defense of involuntariness in cases of impiety as well as injustice. It is also that the standards of extenuation that the Athenians would foist on the gods are amazingly easygoing. The involuntary offenses claiming the indulgence of the gods in this argument are not just those unintentional in the usual sense. Nothing could have been more intentional – deliberate and thoroughly deliberated – than the Athenian seizure of Delion, conceived as part of a master plan for taking over Boeotia (4.76). By *involuntary* the Athenians mean mearly committed under duress – and even *duress* must be taken here in a highly diluted sense. Anything done under pressure of war qualifies as done under duress. If, as in the case at hand, desecration is just good strategy or (as in the case of the use of the lustral water) a necessary consequence of good strategy,[7] this will suffice for a plea of necessity. The deities are called upon to recognize that danger, however incurred, imposes necessities of its own. They must not expect even unjust warriors to lie down and die.

"Is nothing sacred?" bellow the Boeotians confronted with the Athenian desecration of their sanctuary. "No, nothing," the Athenians reply, "not even the sacred is sacred." For it too (if we may assume the reasonableness of the gods) admits of numerous exceptions. Do the Athenians innovate in thus diluting the demands of piety? They claim not. Rather they profess to act on the same time-honored principles as does every other city. Their innovation is confined to the clarity of their grasp of these principles and the candor of their presentation of them. They defend their piety as impeccable, not because they put piety first, but because no one else does either, and because they can argue (however inconsequently) that in the present case the impiety of the Boeotians is worse than their own.

To anyone reading this passage within the context of the work as a whole, the Athenians' approach to piety here will recall their approach to justice elsewhere. Consider, for instance, their initial statement on the question of justice, the speech of their envoys to Sparta on the eve of the war (1.72-78). There too, while adopting a novel candor in speech, they denied that in practicing empire they in any way innovated in deed. There too, without rejecting the authority of justice, they insisted on the propriety of a much broader range of exceptions to it than are generally admitted. There too, arguing from the allegedly universal experience of what humans do as opposed to what they say, they alleged necessity as the ground of these exceptions and adduced necessity as a loophole big enough through which to drive their whole empire.

These arguments about justice Athenians apply to the present issue of piety. The transition from justice is by way of the practice of asylum for involuntary offenders, with its suggestion that the gods indulge involuntary transgression of justice. But must they not then also indulge involuntary impieties? The Athenian thesis naturally proceeds from justice to piety because while these virtues are two, the indicated excuse for neglecting them is one: necessity. Piety no less than justice proves unequal to the most powerful temptations to flout it: judging from what cities do rather than from their criticism of others, these temptations grip them with irresistible force.

It is not hard to see that the Athenian position is self-contradictory. If they may invoke the necessities of war to justify profaning the temple, why should not the Boeotians do so to justify their refusal to yield the bodies? The assertion of the Athenians that the impiety of the Boeotians is graver than their own is insufficient, for the relevant question is not whose offense would be graver in the absence of extenuation but rather which side can offer the more persuasive extenuation of its offense. Here the two parties are roughly equal, it being at least as necessary to the Boeotians to dislodge the Athenians as it is to the latter to retain their foothold. The Athenians' reasoning, if pursued by both sides, could only lead to the disappearance of all piety, as belligerents pitched the threshold for violating it ever lower. By focusing our attention on the exceptions rather than the rule, the Athenians mire the rule in so many exceptions as to strip it of all power as a rule.

And yet it is clear that for the Athenians themselves piety has not lost all authority as a rule. Here too their position is self-contradictory. They remain concerned to recover their corpses. "From the point of view of the Athenian ambassadors to Melos—or of Socrates—the fate of the corpses would be a matter of utter indifference" (Strauss 1964, 208, n. 70). That it is anything but that to these Athenians—that they respond with bitter indignation to what they claim is the sacrilege of the Boeotians while refusing to undo their own—points to a problem with Athenian piety that will loom larger as the war proceeds. The Athenians—to speak very broadly—can live neither with piety nor without it. Without caring to observe its restrictions except where convenient, neither have they purged their souls of the hopes and fears that piety nurtures. To cite only the most egregious case, the Athenians in the assembly, if truly pious, would certainly have heeded the deeply pious Nicias when he urged them to forgo the Sicilian expedition (6.9-14); and that project having miscarried, the Athenian soldiers—were they truly impious—would never have joined that same Nicias in heeding the urgings of moonstruck soothsayers to delay their retreat (7.50), which delay sealed their utter destruction. Their little piety was to prove a doubly dangerous thing.

The crux of the Athenian argument is that even respect for the sacred must yield to the necessities of human life. Obviously this implies that such respect does not itself rank among these necessities. It is worth contrasting this perspective on piety with the traditional one (while keeping in mind the Athenians' claim to be true to tradition). According to worthy graybeards and long-dead poets, the blessings of human life are the gods' to dispense and are therefore contingent upon our piety. Piety persuades the gods to spare us the rigors of (what would otherwise be) necessity (cf., e.g., Hesiod *Works and Days* 232-34; Homer *Odyssey* 19, 109-14; and among the many Biblical parallels, Dt 11:13-21 and Ps 92). In the absence of natural necessity, piety is the only necessity.

Without having thought the matter quite through, the Athenians imply the emancipation of necessity from the gods, or the emergence of absolute necessity. Traditional piety is contradictory, for in recognizing *in practice* certain exceptions to piety, it implies that some things are more fundamental for us than piety. Not that the Athenians go so far as to deny that the gods chastise impiety, wherever it is willful (i.e., wherever it is truly impiety). They do deny, however, that the gods can reasonably expect us to put the sacred first, ahead of the necessities to which we are subject as human beings. The putative justice of the gods consists in their acknowledgment of their stinginess to us. Our situation in this world of which the gods are nominally rulers is not such that we can concern ourselves above all with honoring the gods rather than with coping with the harsh exigencies of which they have failed to relieve us. Unable to oppose our subjection to necessity, the gods should have the decency to acquiesce in it. It is worth stressing that only thus would our necessities become true necessities, necessities in the fullest and harshest sense, from which not even a god can save us—and against which, therefore, piety avails us nothing. Clearly, such an argument subverts divine authority. If the gods are powerless before our necessities, why doubt that they must give way to necessity generally? The Athenian "liberalization" of piety threatens piety, for by suggesting that the gods are subject to necessity it implies that they are less than gods.

This neglected passage in book 4, in which the Athenians first extend their characteristic outlook on international relations to those between god and humankind, is crucial for grasping the logic of their unfolding political theology of imperialism. I mean that doctrine that achieves its zenith in the most notorious episode in Thucydides, the so-called Melian dialogue (5.84-113, esp. 103-5). Our passage is equally crucial, however, for grasping the discovery of the notion of political necessity in the strict sense, that is, of natural necessity as opposed to the radical contingency of a world ruled by gods—a discovery that is the basis of all real political philosophy or science.

Notes

I thank the Earhart Foundation, the National Endowment for the Humanities, and the Social Sciences and Humanities Research Council of Canada for their generous support of my work on Thucydides during 1986-87. I am indebted to Ernest L. Fortin for helpful comments on the substance of the argument and to Malcolm B. Wallace and W. Kendrick Pritchett for their patient advice on points of Greek sacred law and military practice.

1. Such a supplication is rebuffed only once elsewhere in the historical literature of the classical period, namely, Xenophon *Hellenica* 3.5.21-24, where the villains are again the Boeotians (who on this occasion have no sacrilege to adduce against their enemies). See Pritchett 1985, 211-12.

2. Brief mentions of the incident are found in Cicciò 1984, 138-39; Lateiner 1977, 101-2; Pouncey 1980, 94-95; Pritchett 1985, 191-92; Rawlings 1981, 51, n. 46; Schneider 1974, 106, n. 213; and Strasburger 1968, 529, n. 82. Strauss (1964, 208, n. 70), while not discussing the incident at length, clearly indicates the reasons for doing so, Cf. Palmer 1982, 111, n. 19.

3. Reading, with Stahl and Romilly (Thucydides 1963-72), *pro tou eiothosi* ("in the ways already customary") for the *pros tois eiothosi* ("in ways additional to those customary") of the manuscripts.

4. Reading *akousion* ("involuntary") with manuscripts C, E, and F2 rather than *hekousion* ("voluntary") with A, F, and M.

5. Barton and Chavasse (Thucydides 1890, 122-23) decry "the wanton outrage of which [the Athenians] were guilty in seizing and fortifying a holy place and damaging its condition, and the impudent paradoxes by which their act was justified," further noting that in refusing to withdraw from the temple in order to recover the bodies of their fallen, the Athenians "prefer the retention of a political prize to the performance of a pious duty." (In the only other such case on record [see n. 3] the Spartans prefer the pious duty.) Ducrey (1985, 282), Garlan (1972, 37), Lonis (1969, 73), and Parker (1983, 190) agree that the offenses were serious ones, Parker noting in defense of the Boeotians that temple robbers were commonly punished by refusal of burial. In Athens itself the violation of a temple was punishable by death (Demosthenes 24.120), as was, in principle, the uprooting of sacred olive trees (Aristotle, Ath. Pol. 60.2; cf. Lysias 7). On the cutting of sacred vines specifically, see Thucydides 3, 70 and Parker 1983 (p. 165, n. 21). For an overview of the whole question of sacrilege, see Parker 1983 (pp. 144-90). On the violation of temples in wartime, see Sordi 1984.

6. Thucydides' Melians, who have led a life far more sheltered than have the Athenians and Boeotians, imply that piety includes justice (and so that all injustice is impious) (5.104). It appears, however, to have been the predominant view at Athens by this time that only certain human offenses were of concern to the gods, namely the injustices (such as homicide and temple robbery) that were also impieties or specific affronts to the gods. See, most recently, Mikalson 1983 (pp. 27-30).

7. Pritchett (1980, 295-97) suggests that in seizing Delion the Athenians selected what was topographically the best site in the entire region. As he has written me, "It was this or nothing." The availability of the lustral water must itself have been a necessary strategic asset of the place. On the watery bases of Greek sanctuaries, see Panessa 1983.

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Clifford Orwin is Associate Professor of Political Science, University of Toronto, Toronto M5S 1A1, Canada.

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The following articles, controversy, and research notes have been tentatively scheduled for publication in the June 1989 issue:

Alberto Alesina and Howard Rosenthal. "Partisan Cycles in Congressional Elections and the Macroeconomy."

Richard L. Allen, Michael C. Dawson, and Ronald E. Brown. "A Heuristic and Schema Approach to Modeling an African-American Racial Belief System."

Steven L. Burg and Michael L. Berbaum. "Community, Integration, and Stability in Multinational Yugoslavia."

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RELIGION & POLITICS

Liberation Theology: The Political Expression of Religion. By Ricardo Planas. Kansas City: Sheed & Ward 1986. 289p. $12.95 paper.

Liberation Theology and the Liberal Society. Edited by Michael Novak. Washington: American Enterprise Institute for Public Policy Research, 1987. 238p. $14.75 paper.

Planas's book is a balanced, urbane examination of liberation theology that could replace Phillip Berryman's work as the leading descriptive study of the phenomenon. Its objective is to explain the theology with emphasis on its political orientation. It delves into the history, philosophy, and evolution of the movement, which Planas perceives as a reaction against a traditional religion that stresses individualism and private salvation. It is a reaction against a quietism that emphasizes rite and ritual and focuses primarily on the next world. By contrast, liberation theology asserts a this-world religion with a social orientation. It represents a new commitment to the disadvantaged, described as an "option for the poor." It modifies the traditional concern for proper belief (orthodoxy) with a concern for proper action (orthopraxis). Thus conversion now involves dedication to changing the social and economic environment. In essence, liberation theology attempts to reclaim an earlier Christian tradition and spotlight the second of the two great commandments.

Planas perceives liberation theology as a reaction against a European theology that is deductive, elitist, and otherworldly. It is an attempt to develop an indigenous third-world theology that is enriched by dialogue with the oppressed. It stresses the importance of participation through base communities that involve both study and action. It also involves a new terminology, with traditional terms such as *sin*, *redemption*, *idolatry*, and *violence* accorded a totally different connotation. They are now grafted to this world and its transformation.

Liberation theology breaks with European theology in its approach to history; it rejects the traditional dualism. It perceives history as one with no cleavage between the sacred and profane. The world is basically good due to the creation and incarnation; the Fall plays little part in liberationist thinking. The believer has an obligation to sacralize the so-called secular; that is, he or she must act upon the "sinful" social and economic structures in order to promote reform and eventual transformation. The believer must cooperate in building "God's kingdom on earth."

Planas notes that in operationalizing itself, liberation theology has been uncritical in utilizing Marxist theory. Much of the language and prescription smacks of Marx, as does the openness to violence. Nevertheless, he is optimistic about liberation theology's future and its impact on Christianity. He does introduce a few caveats about liberationist naivete and the tendency to pit a theoretical socialism against a capitalist reality. He also expresses some concern for the believers' fate after the revolution.

The Novak book is a record of the AEI Summer Institute of 1985, which was designed to open a dialogue between supporters of Latin American liberation theology and supporters of the U.S. liberal society. It consists of papers on liberation and liberalism, debt crisis, underdevelopment, dependency theory, the language of liberation theology, and economic growth, as well as responses and comments. Despite the surgical orchestration of Novak, the conference's focus seemed to shift from liberation theology to the rationale of development and undevelopment. Again and again participants speculate about the exploitation versus the cultural interpretations of development and undevelopment. Often Japan and the four dragons (i.e. South Korea, Taiwan, Hong Kong, and Singapore) seem of more interest than Latin America and the United States. For students of liberation theology, perhaps most significant is the paper of the prominent Brazilian liberationist, Hugo Assmann, who attempts to dissociate liberation theology from uncritical socialism and dependency theory and adapt it to a democratic environment. Generally, however, while the volume presents a competent summary of reservations about liberation theology, it plows no new ground. Its short essays and spontaneous responses do not lend themselves to academic breakthrough.

Liberation theology to be understood must be ensconced in its proper environment. It is primarily a Latin American, Catholic phenomenon. Most Latin American countries are over 90% Catholic; the indigenous Protestant churches are mainly Pentecostal-fundamentalist and hostile to liberation theology. A majority of liberation theologians are Catholic priests. Hence liberation theology is best studied as a movement within the labyrinth of Catholic tradition.

One would wish that Planas had located liberation theology more firmly within Catholic ecclesiastical history. Although the movement is often cited as a novelty, viewed in historical perspective it has significant antecedents. It might be linked with the Catholic syncretic-enculturation tradition. Catholicism habitually encouraged a rich variety of cultural syncretism. This was possible in that it viewed the New Testament church as the seed of future development rather than the perpetual model. As Catholicism ripped out of the Judaic world, it absorbed accretions from the various cultures encountered. It borrowed from Greek philosophy in developing its Christology, from Roman bureaucracy in evolving its structure, and from Stoicism in elaborating its natural law heritage. It transformed pagan temples into Christian churches and absorbed profane rhythms into its music. The Counter-Reformation Jesuits employed enculturation in an attempt to create native oriental churches that would eliminate Christianity's European accretions. They sought the non-European core of Christianity, which they hoped to mesh with oriental spirituality. The syncretic principle meant that movements initially confronted as hostile were often defanged, baptized, and integrated into the Christian legacy. Augustine integrated Plato, Aquinas integrated Aristotle, and the Jesuits attempted the same with various oriental philosophies. A more recent chapter in Catholic syncretism was the Vatican II Council's incorporation of features of the Protestant Reformation. Liberation theology might be viewed as the latest chapter in the process. This time the effort involves defanging, baptizing, and integrating Marxism. Such syncretic efforts are slow and incremental and seldom without pain and casualties. Aquinas' initial condemnation, the Counter-Reformation "Rites" controversy, the nineteenth-century Americanist struggle, and the career of Teilhard de Chardin bespeaks the dangers involved.

Critics of liberation theology charge that the movement differs qualitatively from historical syncretic exercises. They suggest that traditionally the church absorbed adversaries on its own terms. It reconciled them when they no longer presented a danger to the institution. But with liberation theology the initiative is in a different direction. Christianity is being grafted to Marxism; it is becoming the handmaid of the secular philosophy. There is little trade-off, with the benefits moving in a single direction. Critics charge that liberation theology is sugar-coated Marxism designed by literati to enable rapport with the masses. They see its predecessor as the Deutsche Christen movement rather than the vintage cases. Only time will determine whether liberation theology represents a legitimate manifestation of syncretism.

Liberation theology is also recognizable as a party to a perennial conflict within the Catholic church. This conflict is tolerated as a natural tension that will not be eliminated. It is the tension between the bureaucratic and enthusiastic mentalities or the struggle between the priestly and prophetic principles. The bureaucrats attempt to preserve the structure in a hostile world of limited resources and unlimited demand. For them change is slow, gradual, incremental with maximal concern for consequences. In the best sense, they are a healing and mediating force committed to passing on the tradition intact. By contrast, the prophets are concerned with the original mission of the church and the discrepancy between the ideal and reality. They bemoan the church's compromise with the world and cite its need for reform. The medieval church was adept at containing both the bureaucrats and the prophets within the same institution. It accorded the priests power and relegated the enthusiasts to the monasteries and convents. The enthusiasts would often be canonized as saints but only long after death when they could no longer threaten the system. Dominic, Bernard of Clairvaux, and Francis of Assisi were the most prominent of this species. The medieval church governed itself by practical men of affairs, but allowed some element of dissent to preserve the notion of the ideal. With liberation theology the church again confronts a prophetic movement, that appeals to Christianity's roots and rebels against the limits of the empirical world. It anathematizes the church's alliance with the status quo and seeks to build the New Jerusalem here and now.

Liberation theology, despite its claim to uniqueness, fits into the Catholic tradition in yet another respect. Catholicism since the Council of Trent has stressed both "faith and good works" as the means to salvation. Even before the Vatican II Council, it had not been as quietest and unconcerned with this world as the liberation theologians contend. In Western Europe it erected a vast network of political parties, trade unions, cooperatives, newspapers, schools, hospitals, and self-help organizations. There evolved a vast Catholic "bloc" in most Western countries. Some of this exists in Latin America but seldom on the European scale due to limited resources and the clergy shortage. Probably Planas too readily accepts the liberationist critique of Catholicism as unworldly. Liberationist commitment to church activism does not represent a break with the past but rather a change in thrust and emphasis. It represents a change in focus from the middle-class to the poor and from clerical decision making to participatory democracy.

Even with its apparent roots in Catholic syncretism, the Catholic prophetic convention, and Catholic activism, liberation theology still presents problems for the Catholic tradition. A formidable difficulty is the new theology's disparagement of continuity. The Catholic ethos stresses conformity to a tradition of organismic continuity. Generation builds on generation in something akin to a Burkean chain of being. A doubtful scriptural quotation is interpreted by examining the witness of the ages. How did Irenaeus, Ambrose, Augustine, Aquinas, Bellarmine, Newman, and so on understand the passage? Is there a consensus of the Fathers? If so, one is presumptuous in breaking the tradition and the burden of proof rests with the dissenter. This deference to continuity means few drastic changes or sharp breaks in Catholic theological development. But it is a compliance that carries little weight with the new theologians. For them, history seems to leap from the Old Testament prophets, to the primitive Christians, to the nineteenth century. One searches in vain for references to Augustine, Aquinas, or the medieval scholastics. Centuries of theological development seem to be discarded.

Another major problem with liberation theology is its rejection of traditional Christian dualism, or its attempt to sacralize the secular. This can have disastrous consequences for both state and church. It can lead to a new Caesar-papalism based on a rather fundamentalist interpretation of scripture. Almost by accident, Western Christianity developed a dualistic sense of two autonomous realms, symbolized philosophically by the "two swords." The collapse of Rome and movement of the seat of empire to Byzantium left a universal church in the West to confront the warring tribes. The church possessed a bureaucracy and structure that enabled it to resist integration as a department of state. Unlike Eastern Orthodoxy, it maintained its autonomy, which it asserted throughout the Middle Ages and especially during the papal-imperial conflicts. There evolved the Western notion that there were two realms and thus a sphere outside the control of the state. Later this idea would assume a secular form, but it still involved a dualism, a belief that the "two swords" should not be wielded by the same hand. Separating the secular and spiritual was not without advantage to both state and church. Liberation theology disparages this accomplishment with little appreciation of the consequences of its reversal. The most pessimistic scenario could result in fostering the spirit that Normal Cohn, Ronald Knox, and Leon Festinger find in the studies of millenarianism. Or it could result in a "people's church" based on Eastern Orthodox subservience.

Although heirs of the nineteenth-to-twentieth century Thomistic revival, with its emphasis on realism, and party to a new theology that stresses the inductive, the liberationists as interpreted by Planas seem oblivious of the importance of system survival and system maintenance. They demonstrate little concern for the fate of religious believers and religious structures in Cuba and Eastern Europe. Nor do they inquire why liberation theology holds little appeal for believers in such societies. Until recently, these societies initiated massive efforts to destroy traditional religion or to privatize it, limiting it to rite and ritual. They severely restricted the educational and vocational opportunities of believers. Despite or perhaps because of these activities, Eastern Europe seems to be experiencing a religious revitalization that is affecting Poland, Slovakia, Lithuania, and the Western Ukraine. Although a mass participatory, nonclerical experience, it owes little if anything to liberation theology.

Planas, as well as most commentators on liberation theology, accepts at face value the existence of the base communities. These offer a splendid opportunity for the empirically oriented. Do they really exist outside the imagination of the liberationists? The uninitiated can observe religious fervor in Latin America in the hordes that welcome papal visits, in the human rights campaigns, in the crowded churches, and the unprecedented numbers now studying for the priesthood. They can also observe the proliferation of Pentecostal-fundamentalist churches aimed at the unchurched. But where are the base communities that Assman touts? Surely the one hundred thousand Brazilian base communities must be leaving some mark, somewhere.

Planas is probably correct in predicting that liberation theology will have an impact upon Latin American Catholicism. The religion of today is significantly different from that of a generation ago. Liberation tenets have been incorporated into official documents through the Medellín and Puebla Episcopal conferences. Even in the most conservative churches (e.g., in Columbia and Argentina) there is a new consciousness of the church's social and economic responsibilities. The "option for the poor" is now orthodoxy. But this syncretism has also involved an attempt to liberate the new theology from its uncritical Marxism, its romanticization of violence, and its emphasis on class struggle and balance its concern for salvation in this world with a concern for salvation in the next. Ultimately the fact that liberation theology existed will make it increasingly difficult to use the church and its theology to underpin a traditional establishment. But will the theology in its clerical transformation still be recognizable as liberation theology?

In essence the Planas and Novak books focus on an explosion in Catholic social thought. Catholic philosophy has seldom veered toward utopianism. The belief in the Fall, the church's antiquity, and the fact that Catholic clerics actually wielded power probably deterred such temptation. Catholicism taught that there would be no utopia and the "tares and the wheat" would be separated only in the next world. But Catholic thought also expressed reservations about capitalism. Numerous papal encyclicals of the nineteenth and twentieth centuries questioned the spirit of capitalism under the rubric of "liberalism." Such Anglo-American, Catholic thinkers as G. K. Chesterton, Hilaire Belloc, Eric Gill, and Dorothy Day tried to devise a Christian alternative to socialism and capitalism. Now with liberation theology, there is another—more potent—attempt to redirect Catholic social thought.

DANIEL J. O'NEIL

University of Arizona

Caesar's Coin: Religion and Politics in America. By Richard P. McBrien. New York: MacMillan, 1987. 294p. $19.95.

Politics, Power, and the Church: The Catholic Crisis and Its Challenge to American Pluralism. By Lawrence Lader. New York: MacMillan, 1987. 273p. $22.95.

The persistance of religion in U.S. society and culture and the increased political activism of religiously committed citizens during the 1980s have challenged political scientists to attend to the consequences of religious belief for U.S. public life. The two books under review here examine the relation between religion and politics in light of the advent of evangelical Protestantism and U.S. Catholicism onto the stage of public affairs. Lader fears that a coalition of Catholics and fundamentalists in alliance with Reagan administration Republicans "has threatened the American tenet of church-state separation and shaken the fragile balance of our pluralistic society" (p. 11). McBrien, who is generally more positive about the political activism of the churches, seeks in his book to specify the many different aspects of the relation between religion and politics. Since both authors focus primarily on U.S. Catholicism, it is appropriate to recall some recent Catholic history.

Since the Second Vatican Council (1962–65), the U.S. Catholic church has assumed an increasingly prophetic stance, issuing forceful, comprehensive statements on war and peace and on economic justice that are critical of the conventional, mainstream views of U.S. domestic and foreign policymakers. The U.S. bishops have monitored policy in Central America, for example; they have also been highly critical of *Roe* v. *Wade* (1973) and have pressed actively for the reinstatement of restrictive abortion laws. In assuming this new public role, the Catholic church, together with other denominations, has asserted and defended the right of religious institutions to address the moral dimensions of public issues.

Lawrence Lader contends that "while Catholic bishops and fundamentalists certainly have the right under the First Amendment to preach and educate on any issue, the question is whether their power should be used to push their beliefs on the majority through legislation without a national consensus" (p. 10). However, rather than confine himself to a substantive analysis of one or several issues illustrating this central contention, Lader addresses many issues: birth control, abortion, aid to parochial schools, homosexuals' rights, Catholic hospitals and medical ethics, Vatican finances, liberation theology, Catholic bishops and the ERA, school prayer, the bishops' pastoral letter on war and peace, the sanctuary movement, and the bishops' statement on economic justice. It soon becomes clear that for Lader, any efforts by the Catholic church to shape a public consensus on any issue are inherently suspect as possible violations of church-state separation.

Lader claims that "the development of Catholic power—the influence of its religious morality and political aims on U.S. society—has followed a careful design," expanding from local urban power in the early 1900s to national power in the alliance with fundamentalists during the Reagan years. However, while the bishops share some concerns with fundamentalists on such issues as abortion, the rights of homosexuals, and the integrity of independent schools, the U.S. Catholic Church differs from the religious Right on other issues. For example, Lader seems to ignore the bishops' critique of the Reagan administration's Central American policy as well as their opposition to capital punishment (another item on the conservative agenda). Lader acknowledges that the Catholic hierarchy, in its pastoral letter on war, abandoned support for the Reagan administration's nuclear buildup; yet he suggests that the bishops failed to follow up the letter with political pressure on the administration because they were unwilling to upset their alliance with fundamentalists and New Rights Republicans. No evidence is offered to support this conjecture. He also ignores major theological differences between Catholics and fundamentalists (e.g., on ethics or on biblical interpretation) that influence how each religious group views the translation of religiously based moral norms into public policy.

Moreover, Lader asserts that Catholic power is to be feared because "the separation principle has always been alien to the church, and the Vatican, in large part, has failed to accept it" (p. 232). Here, unfortunately, Lader fails to take account of the work of the U.S. Jesuit, John Courtney Murray, and of the 1965 *Declaration on Religious Liberty* of the Second Vatican Council. He ignores the fact that in Hungary and Poland today Catholics strongly endorse the principle of church-state separation in order to protect the religious freedom of citizens and churches.

Although Lader's account is a useful chronicle of dissent within the U.S. Catholic church, the scope is too broad; he also makes unsubstantiated inferences regarding the church's influence in U.S. politics. Moreover, Lader never defines pluralism and never examines analytically or historically the principle of separation of church and state. He seems to assume that the meaning of these terms is self-evident. Clearly, his treatment of these complex issues would benefit from the more precise distinctions McBrien carefully makes.

McBrien's book grew out of the 1984 presidential election campaign and the confused debate over the relation between religion and politics that figured so conspicuously in that campaign. In seeking to clarify the terms of the dialogue about religion and politics, McBrien distinguishes four levels of recent debate: the relations between morality and politics, religion and politics, church and state, and clergy and politics. He contends that morality and religion are overlapping but not identical or coterminous. In fact, he argues, "failure to perceive the difference between morality and religion is at the root of most of the confusion in debates about religion and politics in general, and about specific issues like abortion" (p. 204).

McBrien's book is "intended for intelligent nonspecialists, and especially those for whom devotion to 'the public good' is no idle pursuit" (p. ix). That his book is not intended for scholars seems obvious in his discussions of political philosophy, which are somewhat superficial. His analysis of liberal political thought is especially deficient; he seems unaware of the ongoing discussion by scholars of the relation between the liberal polity and individual virtue. On the other hand, in the middle chapters McBrien provides an extended analysis of church and state in U.S. history and law and a very good summary of judicial rulings on the First Amendment religion clauses.

In the last section of the book McBrien applies his careful distinctions to a variety of cases, including school prayer, tax exemption, military chaplaincies, school aid, the sanctuary movement, gay rights ordinances, and pornography. In the longest chapter, he tackles "the hardest case," abortion. He provides an exhaustive, detailed analysis of the debate about abortion policy in the 1984 campaign, focusing on the statements of Governor Mario Cuomo, vice-presidential candidate Geraldine Ferraro, and various Catholic bishops. Clearly sympathetic to Cuomo, McBrien stresses the search for a public moral consensus on the abortion issue, and endorses Cuomo's argument, made in his public lecture at Notre Dame in 1984, that government cannot act in the absence of such a consensus. Still, McBrien wonders whether political leaders do not have a duty to help shape a consensus or exploit the degree of consensus that may already exist. In fact, McBrien claims, there exists in U.S. society an implicit consensus against abortion-on-demand and in favor of a moderately restrictive law that would permit abortion in cases of rape, incest, danger to the mother's life, or radical fetal deformity. Presumably, Catholic citizens and politicians should help to articulate this hidden consensus.

Since abortion is a case study of the relation between religion, morality, and public policy in U.S. society, it is appropriate here to discuss McBrien's argument for a moderately restrictive law. McBrien contends that abortion is a moral rather than a sectarian religious issue, that it is not a matter of private morality but an issue of public morality subject to civil law, and that a natural law approach can lead to consensus about appropriate public policy on abortion. He argues that the obligation to respect human life at every stage, including the fetal, is a moral requirement rooted in right reason and binding upon all persons. But this statement of moral obligation is so general as to afford little guidance to those who must apply such general principles to particular complex situations. Moreover, McBrien and other natural law proponents must take seriously the fact that despite the claims of right reason as an ethical tradition, reasonable persons simply do not agree about the status of fetal life, the scope of women's moral agency, the moral permissibility of abortion, or the use of criminal and civil law as mechanisms of abortion regulation. In other words, the natural law approach favored by McBrien does not seem to possess any special advantage over other forms of ethical reasoning in developing a public consensus on the morality and legality of abortion.

McBrien's distinction between state and society might alert him to the possibilities for churches and other social institutions to fashion a shared moral consensus without necessarily translating that consensus into law on such a complex issue as abortion. Perhaps McBrien should follow his mentor, John Courtney Murray, who argued that civil law "enforces only what is minimally acceptable, and in this sense socially necessary. Beyond this society must look to other institutions for the elevation and maintenance of its moral standards—that is, to the church, the home, the school, and the whole network of voluntary associations that concern themselves with public morality in one or another aspect" (quoted by McBrien, p. 165). Conceivably, this conception of the relation between law and morals is a view on which McBrien and Lader might possibly agree.

In general, McBrien's book is more valuable than Lader's as a text for classroom use and as a comprehensive overview of the many aspects of the relation between religion and politics. McBrien notes the importance of religion in U.S. society as an institution engaged in the tasks of character formation so necessary for public life. Whereas Lader generally conceives religion to be inimical to human freedom and civility, McBrien's view of the interrelatedness of religion and politics suggests that religion in the United States should be nonestablished but also nonprivatized. One suspects that this generous, broad-minded view of the role of religion in public life is also one more hospitable to U.S. pluralism.

MARY C. SEGERS

Rutgers University
Harvard Divinity School

Religious Resurgence: Contemporary Cases in Islam, Christianity, and Judaism. Edited by Richard T. Antoun and Mary Elaine Hegland. Syracuse: Syracuse University Press, 1987. 269p. $27.50.

Islam in Asia: Religion, Politics, and Society. Edited by John Esposito. New York: Oxford University Press, 1987. 272p. $29.95 paper.

The Catholic Church in World Politics. By Eric O. Hanson. Princeton: Princeton University Press, 1987. 485p. $24.95.

Cross and Commissar: The Politics of Religion in Eastern Europe and the USSR. By Pedro Ramet. Bloomington: Indiana University Press, 1987. 244p. $22.50.

The four books under review are only a sample of the cascade of books on religion and politics published in the last five years. They touch every continent save Antarctica and discuss a wide variety of religious expression in Orthodoxy, Islam, Judaism, and Christianity (both Protestant and Catholic).

Each work under review, with the exception of the Esposito collection, is intended to make a contribution to the theoretical literature in these fields. Yet despite their numerous values, they fail to advance general theoretical understanding of the relationship between religion and politics, nationally or cross-nationally, though they do provide considerable raw material for theorizing. The most stimulating theoretical speculation in the field remains Donald Eugene Smith's *Religion and Political Development* (1970), now almost two decades old.

Antoun and Hegland put their collection within the context of religious revival movements throughout the world. Islam in the Middle East is not unique, and it is refreshing to have it presented in the context of other religions in other parts of the world. Among the articles on Islam, the most noteworthy is that by John O. Voll. His analysis, "Islamic Renewal and the 'Failure of the West' " gives new perspective to the often-repeated truism that Islam does not recognize separation of church and state. Voll presents a careful historical exegesis of the growth within Islamic thought of the idea that the West has failed. With this failure the appeal of Western secularism, very real within Islam, decayed. This decay, somewhat different from nation to nation, has substantially influenced the specific forms taken by Islamic revivalism.

Robert Wuthnow contributes a very solid summary of the evidence concerning religious resurgence in the United States, concluding that there is not a general religious revival going on but rather a surge of press interest in religion and a revival *within* the specific religion of fundamentalist Christianity. This revival, he argues, poses little threat to U.S. institutions. Mahmoud M. Ayoub compares traditions of martyrdom in Islam and Christianity, but briefly and without drawing out its importance to current revivals.

The key theme and theoretical perspective of the book is that revival movements in the United States, Israel, and Islamic nations are modern, not traditional or reactionary. These movements envision an active role for government to bring about improved social, economic, and political conditions. This thesis, stated best in Mary Elaine Hegland's conclusion that "enabled" rather than deprived groups have participated in and stirred religious revivals in both developed and underdeveloped nations, is an important one. Antoun and Hegland marshal the evidence for it well.

Islam in Asia, edited by John L. Esposito, was prepared as part of a project organized by the Asia Society, New York. Its primary purpose is to introduce nonspecialists to the diversity of Islamic life in Asia, defined as running from Iran to the Philippines. The book intends to break down stereotypes and to balance the usual focus of the Middle East. The focus is principally on the role of Islam in the public life of Asian nations.

In keeping with the book's primary intention, the articles are almost entirely descriptive and categorical, rather than analytical or theoretical. The descriptions are largely institutional and legal concerning the role of Islam in public life. The fundamental theme and organizing principle of the book is the diversity of Islamic political expression, a theme chosen to counter the stereotype that Islam insists on the unification of church and state and the development of Islamic republics in all nations.

The first set of articles describes Islamic republics, in which the nation is officially Islamic and Islamic law is embodied in constitutional and statutory law. Shahrough Akhavi describes the Iranian Islamic state, and Kemal A. Faruki addresses the case of Pakistan. Each is an almost purely journalistic summary of the contemporary situation in these nations, but Faruki does a fine job of indicating how difficult the contemporary world makes it to institutionalize Islam.

Islam can also take the form of resistance movements, and two of these are described: Afghanistan (by Ashraf Ghani) and the Philippines (by Lela Garner Noble). In yet other cases Muslims live as religious minorities in relative peace. John O. Voll considers this situation in China and Soviet Central Asia. He argues convincingly that certain mediating structures challenge the traditional claim that Muslim minorities have only two alternatives: emigration or holy war. Such structures are informal devotional groups such as Sufi *tariqahs*, nationalities, and even the formal official establishments. Voll argues that such structures have allowed Muslims to react quite flexibly to their minority situation in China and the USSR and to preserve their faith in the face of official atheism. Syed Shahabuddin and Theodore Paul Wright, Jr. undertake a similar analysis of Muslims as a minority group in India.

The fourth case is one in which Islam has accommodated in various ways to religious and cultural pluralism. In Malaysia (Fred R. von der Mehden) and Indonesia (Anthony H. Johns) Islam, though dominant, must coexist with other religions strongly rooted in the culture of different ethnic groups. In addition to these articles, there is a good, clear introduction by Esposito and a solid account of the international relations of Islamic nations by James Piscatori.

Pedro Ramet does a fine job of describing the great variety of religiopolitical interactions in Eastern Europe and the USSR, particularly the ways in which the national histories of each country and the role of religion in those histories have produced persecution in some situations, accommodation in others, and cycles of oppression and resistance in still others. He integrates history and current events well, though sometimes the shifts in focus between and even within chapters become confusing. Also useful is the account of factions within different religious groups and within certain governments on the religion question, in Yugoslavia, for example.

Less satisfactory is Ramet's attempt to be theoretical. Each of the early chapters has a different theoretical focus: functional analysis (Soviet context, chap. 3); "geneticism-monism" (chap. 4 on Polish, Czech, Slovak, and Hungarian Catholicism); political culture (chap. 5, East Germany); factional analysis (chap. 6, Croatia); and organization theory (chap. 7, Yugoslavia and Bulgaria). Since a number of these chapters were originally published elsewhere for different types of audiences and since the "theories" are introduced very briefly and then abandoned in favor of a largely historical and descriptive method, the theoretical perspectives do not advance the analysis and ultimately become annoying and confusing.

In this light, *Cross and Commissar* serves best as primer on the religious situation, movements, and changes in Eastern Europe, and on the factors contributing to the relative strength and weakness of religious and political forces.

Eric O. Hanson's *The Catholic Church in World Politics* attempts to integrate the study of the Catholic Church at the national level with its role in international politics. To do so, Hanson relies principally on country and regional studies by other authors, which he hopes to weave together with the international focus that he himself develops. His specific international concern is national security policy and arms control. A second goal of the book is assessment of the impact of modern political and technological developments on the internal and external politics of the Church.

As with the Ramet and the Antoun and Hegland books, a good distance separates goals and achievement. Despite a methodological appendix situating social science studies of the Catholic church and appealing to the "eclectic" methodology used by Robert Bellah and his colleagues in *Habits of the Heart* (1985), Hanson's methodology is not systematic in the way Bellah's is. He is, instead, dependent on the varying methodologies of his sources. Moreover, his fundamental concepts are not carefully defined, and he fails to generate convincing conclusions about his primary themes.

Hanson is altogether too fascinated with detailed reporting on specific events and personalities to present clear hypotheses, evidence, and conclusions about the principal themes. The book as a whole focuses on the international relations of the Catholic church as an organization, providing minimal new insight into the church's effect on U.S.-USSR relations. Even chapter 9, which is to summarize the conclusions of the book, focuses on the superpowers' reaction to church activity within their client states, rather than on the direct relations between the United States and the Soviet Union with respect to national security and arms control. Hanson provides important information about the Catholic church's activity in many parts of the world, but it does not fulfill the goal of elucidating the effect of religion (specifically Catholic) on the relations *between* nations.

There is considerable valuable descriptive material in the book about how individual church leaders, both within specific countries and at the Vatican, have approached international issues and about how specific institutions, especially the Curia and certain national episcopal conferences, have done so. Hanson pulls together between two covers a wealth of material about the current political activity of the Catholic church, not a minor accomplishment. But there are no strong conclusions of a theoretical sort regarding international politics until the final chapter, and even these remain largely speculative and not tied firmly to the previous chapters. Moreover, the subject of technology is very weakly handled. It appears in only two fashions, as modern weapons technology (where it is redundant with the first theme of the book) and as media accounts of various happenings involving the church, especially the travels of Pope John Paul II. Hanson is fascinated with how the news, particularly the *New York Times*, covers church activity in the international sphere. But this concern does not produce notable hypotheses or evidence about technology generally.

Beyond the strengths and limitations of these books considered separately is the forceful impression of common issues and themes conveyed when they are read together.

Perhaps the most weighty common theoretical question is one prominent in the literature on the New Religious Right in the United States; that is, whether the international religious revival is primarily rooted in a defensive reaction by deprived groups against alienation and marginalization or whether it is, as Hegland argues, a phenomenon of rising status groups (Antoun and Hegland, Intro. chap. 11). Her argument that religious revival is connected to rising status is a necessary corrective to the often-too-quickly-assumed opposite hypothesis. Yet her evidence is not overwhelming, and the article by Davis in her own volume suggests otherwise, at least for Egypt. Moreover, Ramet's account of religious revival in Eastern Europe (esp. chap. 8) does not support a generalized connection between rising status and religious revival. Of course, there is no reason why religious revival could not be stimulated by *both* falling and rising status. The fundamental problem would be to discover the conditions under which each might generate revival and whether the types of revival would be similar and different. Moreover, one might examine different religions or the same religions in different types of societies to see whether certain religions tend to experience resurgence in falling or rising status conditions. Also, the significance of religious and political leadership cannot be overlooked, as all four volumes amply demonstrate.

The themes just discussed are as much sociological as political, and the relatively more advanced research on religion in sociology and anthropology may account for their prominence. Another theme, however, raises a fundamental question for political science. Religion in the contemporary world has become a political resource for resistance to oppressive regimes of the Left and Right, though not universally so. As Ramet points out, the Orthodox churches have largely accommodated themselves to Communist regimes. It is, however, the resisting churches (e.g., Protestant sectarian groups in Romania, pp. 160-63) that are gaining members, while the Orthodox church in Romania is not experiencing a resurgence.

Latin America is a striking example of how religion has recently served as the focus of opposition and resistance to oppression in nations like Brazil, Chile, El Salvador, and Nicaragua (Hanson, chaps. 2-3, 7). The experience, however, is more general. Ramet discusses how Catholicism in Poland has focused resistance to the regime, how the evangelical church in East Germany has stimulated and supported a peace movement very troublesome to the government, and how the "secret church" within Catholicism in Czechoslovakia has doggedly resisted state pressure. Esposito's collection of essays on Islam in Asia evidences Islam's contemporary resistance to oppression. In Antoun and Hegland, this theme of resistance to oppression or, more modestly, religion as catalyst of opposition to dominant political power, appears in a number of essays.

Although these volumes provide important insight into the dynamics of religion and resistance, they supply no systematic account of what factors in a religion produce either accommodation or resistance, as well as no systematic account of the political, economic, or social conditions that might push religious groups in the direction of resistance to dominant regimes, especially oppressive ones. Moreover, since we know that religion can also serve to legitimize oppression, the most significant theoretical issue would be to explore the conditions internal and external to religion that move it in different directions.

Hanson deals with this question in terms of legitimacy, suggesting that societies need what he calls a "primary ethical broker." Where political forces lose legitimacy, relying more on "instrumental" than "expressive" power, they forfeit any claim to being the primary ethical brokers. The Catholic church, Hanson argues, can and has come to fill the ethical gap, pulling legitimacy away from the regime in certain nations. Hanson's evidence, however, is not strong or systematic. He does not provide strict definitions of key terms, nor does he furnish systematic evidence of their presence or absence in particular situations. His judgments about the presence of a primary ethical broker and of degrees of expressive and instrumental power are both impressionistic and post facto. The concepts are so far only theoretically suggestive.

Religion and nationalism is a major theme in all four works, a theme bearing on both the reasons for religious resurgence and on religion as resistance. It may be that religion is serving in many nations as a surrogate for, or as a symbol of, deeper nationalistic forces. The danger is that research in "religion and politics" may emphasize one of the terms—usually the former—to the neglect of the other. The temptation is to stress religion to the neglect of political ideology, culture, nationalism, and economics. The corresponding danger, for most research in comparative politics, is to consider nationalism as a primary independent variable, with religion as an epiphenomenon.

We do learn from these books far more about religion in the countries and regions described than we do about political forces, or about the dynamics of religious-political interaction. Yet, to their credit, the authors are aware of the problem, and they illumine quite well the specific political interplay of religion and nationalism.

Ramet does the best job in this respect. Nationalism is a major theme of the book. He argues that at least as far as Eastern Europe and the Soviet Union are concerned, a nation's religious policy is strongly influenced by certain nationalistic factors: a religion's allegiance (if any) to a foreign authority, its behavior during World War II, the ethnic configuration of a country, and the dominant political culture: "Whether a religious organization is suppressed, tolerated, or coopted depends in part on its nationalist demeanor and the significance of this demeanor in the context of the country (specifically, is the country ethnically homogeneous or multiethnic?)" (p. 187). This hypothesis can also be broadened beyond the issue of a regime's religious policy. These factors will also affect the chances for a religious resurgence and of a religion's taking the lead in, or serving as the focus of, resistance. The other authors do not put this question as directly as Ramet, but they provide sufficient evidence to strengthen his conclusion.

Hanson addresses the different national character of the Catholic church in each country. Reforms since the Second Vatican Council in the 1960s have placed significant new emphasis on national and regional conferences of bishops. Thus the national character of the various churches will say much about their implementation of Rome's decisions and about their interaction with political actors: "Current analysis of Catholic politics must seek to integrate the study of Catholicism in its national contexts with an understanding of the church at the international level. Such integration poses the special challenge of this book" (p. 14).

Antoun and Hegland include a number of articles specifically focusing on the integration of religion with modern forms of nationalism. The articles in Esposito come closest to focusing on religion to the neglect of forces such as nationalism.

These are four useful additions to the literature on religion and politics, particularly for students or scholars who are not specialists in the field or who want information about countries, regions, or religions with which they are not intimately familiar. Each book contains a very good bibliography and index, as well as extensive footnotes directing attention to specialized works on every topic covered. The wealth of research they draw on marks the significant progress made in the study of religion and politics in the last 10 years. Their high theoretical ambitions and modest theoretical achievements chart how far the field still has to go.

CLARKE E. COCHRAN

Texas Tech University

POLITICAL THEORY

Toward a Theory of Eurocommunism: The Relationship of Eurocommunism to Eurosocialism. By Armen Antonian. New York: Greenwood Press, 1987. 188p. $35.00.

Eurocommunism is a term loosely used to describe the political changes in the Italian, Spanish, and French Communist parties during the 1970s. Until recently, Eurocommunism has been studied mainly from the perspective of comparative politics, mostly focusing on the examination of a single Communist party or else from the angle of international relations, emphasizing strategy and foreign policy considerations. In this book Armen Antonian attempts to provide a definition of Eurocommunism, to treat it as a political ideology rather than just a movement, and to go beyond the usual studies of this phenomenon by addressing the topic from the point of view of political theory as well as historical analysis.

The great strength of this book is the synthesis of large amounts of historical data and Antonian's ability to develop the relationship of Eurocommunist theory to socialist thought, classical liberalism, and East European dissident analysis of the USSR. Examining the histories and ideologies of the French, Spanish, and Italian Communist parties, Antonian argues that Eurocommunism represents a unique brand of communism stemming from the split of the European Left into socialist and communist factions predicated on differing views of the Bolshevik Revolution. Moreover, Antonian insightfully recognizes that Eurocommunism combines its critique of capitalism and vision of classless society with major elements of the liberal tradition, including the separation of powers, universal suffrage, free elections, and so on. Thus, Eurocommunism is more than just a movement based on the short-term policies of any one political party; it has become part of the general political discourse of the European Left.

In his historical analysis of the political parties, Antonian also uncovers the important role that the Nomenklatura system has played in the French Communist Party (PCF). *Nomenklatura* normally refers to the privileged executors of the state or special bureaucrats who direct the operations of a ruling Communist party, as in the USSR. Here, Antonian finds that a similar phenomena has developed within the PCF and in fact has helped to push the party away from Eurocommunism in the 1980s.

For the reader who is unfamiliar with Eurocommunism, Antonian's work will provide an excellent discussion of the historical roots of communism in Western Europe. His ideas are expressed clearly and are well organized, making the complexities of historical detail and the logic of Marxist thought manageable for the student.

However, political scientists and historians who are experts in this field will be somewhat disappointed. Antonian synthesizes previous analyses of Eurocommunism and provides the reader with little that is new. While some primary sources are used, such as statements and speeches from party congresses, much of the material in the book comes from secondary sources. Aspects of his analysis have also been developed elsewhere. For example, Santiago Carillo in *Eurocommunism and the State* (1978) discusses the split from the Leninist view of communism, and Arthur Hirsh in *The French New Left* (1981) addresses the issue of Eurocommunism and the PCF, placing the French version of Eurocommunism in the context of French political thought.

The author periodically refers to members of the Spanish, Italian, and French Communist parties as if all of these individuals were male. Given the historical and growing importance of the role of women in each of these organizations, it is surprising that the book's language does not reflect this.

While Antonian does not uncover new documents to enhance his analysis, his work does go beyond the framework of a case study of a single Communist party. He is able to derive a theoretical statement about Eurocommunism, synthesizing much of the previous research on the topic. As a work that deals with the history of the ideology as well as its relationship to political thought, this book should be valuable as a contemporary reference for courses on Marxism, Western European politics, and modern political theory.

KAREN J. VOGEL

Washington State University

On Classical Liberalism and Libertarianism. By Norman P. Barry. New York: St. Martin's Press, 1987. 215p. $29.95.

This book attempts to present "a general account of the various philosophical strands that go to make up the liberal tradition" (p. ix). The author asks us to see the work less as a historical work and more as a collection of essays on a particular theme. The main finding is that the fundamental premises of the individualist tradition are as diverse as the broader tradition of social theorizing. The chapters deal with the role of rationalist and empiricist, utilitarian-consequentialist, and rights-based views and of subjectivist-noncognitivist and objectivist-naturalist approaches in the justification of social order dedicated to maximizing individual freedom.

Classical liberalism is identified as an "economic consequentialist" approach—a tradition of theorizing rooted in the writings of David Hume, Adam Smith, and the utilitarians. (Bentham is set aside because Barry's main concern is with the theoretical frameworks that are unable to justify state interventionism.) Chapters deal with the Chicago school of empiricists, the Austrian school—particularly von Mises, but also Hayek—and James Buchanan's "Contractarian Liberalism." These approaches are contrasted with a libertarian tradition that appeals to natural rights and natural law in the justification of social institutions and policies. It is asserted that the term *libertarian* would be unnecessary if it were not for the fact that in the United States the term *liberalism* has been captured by "collectivists." That these terms are merely handy labels to mark points on a continuum of individualist concern is highlighted with the inclusion of a chapter on the "anarcho-capitalism" of Murray Rothbard, which does not fit neatly in either category. The libertarian tradition is represented by a chapter on the deontological "rights" approach of Robert Nozick and one on the teleological "natural law" objectivism that Ayn Rand uses in justifying egoism.

The book is for those who already know, at least in outline, the positions on social theory and policy of each of these authors; for the book devotes itself primarily to the task of explicating, comparing, and contrasting the philosophical foundations of these approaches—the apparatuses for political and moral justification. The assertion of a model of an autonomous individual and the demonstration of the efficiency of the market—juxtaposed to show the blossoming of individual free choice with general prosperity—is taken by some authors as sufficiently persuasive to skip the stage of philosophical justification, which would involve exposing the grounds on which they claim valuable political knowledge.

The attempt to sort out approaches to the ethical grounding of individualism should provide an opportunity to explore various dimensions of the concept of the individual implicit in the varieties of liberalism and libertarianism. Critical comments on the "passive" concept of the individual in Chicago school, related to the consistent empiricism of that school, whet the reader's appetite. But regrettably, that line of inquiry is not developed. Nor are the implications of the naturalist approach for developing a concept of the "person" as a focus for individuality explored. In particular, it seems important to see how these theoretical approaches do, or could, respond to the problem of the intersection of social institutions and the processes of social change. It is in dealing with social change that we must come to grips with the individual as a social human being. In his conclusion, Barry claims that in the liberal tradition "the individual is never abstracted from social processes, either as a rights-bearer or, even, as a utility-bearer." To explore that claim would have been a worthy endeavor. The author is not concerned with this development because there are plenty of critiques of individualism "available in collectivist sources" (p. x).

The curtailment of the scope of these essays may be related to the intention of the author to take an "internal point of view" (p. x) from within this liberal tradition. It is never quite clear where the author stands within that tradition. But that is not so important, for in such a sympathetic review, these approaches can be juxtaposed in a way that allows each to interrogate the others.

The book should stand as a useful reminder of the variety of philosophical directions that can be taken in the justification of an individualist theory. Its most important contribution could be to impress on the reader that such a justification is necessary.

ROBERT PARKS

Elmira College

Which Socialism? Marxism, Socialism, and Democracy. By Norberto Bobbio. Translated by Roger Griffin and edited and introduced by Richard Bellamy. Minneapolis: University of Minnesota Press, 1987. 242p. $35.00 cloth, $14.95 paper.

Since the nineteenth century, socialists have argued over whether they should struggle for power within the framework of liberal democracy, and if so, whether that framework should be discarded as a remnant of capitalism when they come to power. In most Western democracies such questions have now been relegated to the fringes of political debate by the predominance of reformist socialist or social democratic parties, but in Italy the strength of the Communist party and the weakness of social democracy has kept the issue of liberal democracy very much alive. In this volume of essays, a leading Italian political philosopher and Socialist party theoretician, Norberto Bobbio, disputes Marxian socialism and elaborates a case for reformist socialism founded on the defense of liberal freedoms and representative democracy. Nine essays written by Bobbio between 1968 and 1978 are reproduced in this volume along with a response by Antonio Negri.

Bobbio's defense of representative democracy is based on a skeptical analysis of the principal alternative, direct democracy, the conclusion of which is that however valuable direct democracy might be as a supplement to representative democracy, it cannot replace it. Attacks on representative democracy in the name of direct or grass roots democracy have taken place, Bobbio argues, in an era unpropitious for all forms of democratic control. The increasing scale and bureaucratization of political life, the ever-expanding demand for technical knowledge in decision making and the rise of a conformist mass culture make the realization of representative democracy increasingly difficult and direct democracy harder still. This dilemma, Bobbio emphasizes, is faced by socialists no less than liberals. The harsh lessons about the abuse of political power that liberals have grappled with for two centuries and their conclusions about the need to defend individual rights and strengthen representative democracy must be heeded by socialists as well.

The second major theme of these essays is the weakness of Marxist analyses of the state capable of guiding the practical choices that have to be made concerning alternative governmental structures. If the potential for domination of man over man does not disappear under communism, as Marx and Lenin believed it would, then socialists must think concretely about which political institutions provide the best framework for democratic participation, defense against potential abuse, and freedom of dissent. If this is the case, it is no use going back to Marx's meager writings on the subject and combing them, once again, in the hope of unraveling immutable truths. Bobbio scathingly attacks this kind of scholasticism as fundamentally alien to the critical spirit. He goes on to criticize Marxists for their failure to analyze liberal freedoms independently of the development of the capitalist economic system in which they arose. This tendency has had the perverse consequence of justifying the view that guarantees of individual rights are not necessary once capitalism has been transcended. Antonio Negri's essay in response, which romantically maintains that given the intensifying proletarian struggle, it is pointless to ask what will take the place of representative democracy, is a weak point in the volume and only serves to strengthen Bobbio's claims.

Bobbio has the passionate commitment to representative democracy and individual freedom of one who has seen the results of uncritical faith in human perfectability. As Bobbio states in an essay on Marxism and socialism, while he has great respect for those faiths that can move mountains, he cannot forget that mountains moved in this way have sometimes buried entire populations under their debris. Bobbio's criticisms of Marxian socialism created quite a stir when they were published in Italy in the 1970s. These essays were designed with a political project in mind: to place the Socialist party in the mainstream of Italian politics by emphasizing the distance between socialists and the Communist party. But the issues Bobbio raises are universal, and his treatment of them should be of interest to any one concerned with the underpinnings of reformist socialism in contemporary Western society. Bobbio's defense of reformist socialism is sensible and urbane, but it is not systematic. The reader will have to turn elsewhere for a sustained analysis of the foundations, strengths, and weaknesses of alternative conceptions of democracy. These essays give the reader an opportunity to follow a world-wise philosopher coming to terms with the relationship between socialism and liberal democracy with a zest tempered by a deep sense of historical perspective.

GARY MARKS

University of North Carolina, Chapel Hill

Meeting Needs. By David Braybrooke. Princeton: Princeton University Press, 1987. 344p. $35.00 cloth, $12.50 paper.

This work is a vindication of the idea that social policy can and should be guided foremost by the concept of needs rather than preferences. But why, we might reasonably ask, should anyone feel it necessary to defend the concept of needs? Meeting needs is, after all, what political candidates claim to have done or promise to do every year.

The author's point is that meeting needs no longer ranks as the primary role of government in Western democracies. Ancient and medieval writers certainly proceeded on the principle that needs were primary and preferences less so on the governmental agenda; however, with the Enlightenment the emphasis on free and rational choice as characterizing the good life heightened the role of individual preference in political thinking—not intentionally at the expense of needs, but it worked out that way. In Locke's designation of property as a natural right and even more in Jefferson's formulation of "pursuit of happiness" as a natural right the stress is more on what people want than on what they or others need. Braybrooke points out quite correctly that liberalism emanating from the Enlightenment and conservatism in the English-speaking world stressed rights and choices so much as to leave the concept of needs to Marxists and other advocates of egalitarian schemes. He devotes nearly a chapter to demonstrating that needs are still a central philosophical consideration in systems that promise equality and figure less prominently in systems which focus on individual rights.

Roughly a third of the book analyzes reasons why modern political economists downplay the concept of needs. Most of these are reducible to the conviction that needs are really subjective, vary widely from society to society, and are very difficult to measure. By way of contrast, preferences can be fairly measured in a free society by what people choose to support in platforms and programs, even though preferences may be even more subjective and varied than needs. In short, the market principle takes care of preferences much easier than needs.

Braybrooke attempts to show that the concept of needs can indeed be made less vague and subjective by postulating that each individual in modern society must be able to fill the roles of parent, householder, worker, and citizen. He finds it quite possible to delineate what is necessary for people to function in each of these roles in a manner satisfactory to themselves and to those affected by their role performance. His "List of Matters of Need" includes basic physical needs and the social needs that have been generally recognized in the modern English-speaking world and beyond as essential for role fulfillment to be satisfying, such as companionship, education, and recreation. Braybrooke is perfectly willing to let preferences and differences in the goals of various societies play their parts in setting priorities and minimum standards. The key thing for him is that real, role-essential needs be met before others. In the formulation and implementation of his needs-based agenda, he sees two basic units, the "Selfgovliset" (self-governing linguistic group) and the reference population. The point is that modern democracies perceive needs outside their own citizenry, particularly in the third world: the reference population is the whole group whose needs should be met by the Selfgovliset, according to minimum standards.

Braybrooke has a unique style in his presentation, which takes a bit of getting used to but is fairly engaging after a while. His frame of reference weaves back and forth in appearing variously as mankind in the modern world, Western democracies, or the English-speaking community, but this seldom obscures his points. Some of the chapters are scholastic in the thirteenth-century sense, pursuing the truth by presenting and supporting, but eventually shooting down the wrong answers to good questions until the winnowing-out process yields only right answers. Dialogues with an imaginary "critical reader" are interspersed in another modernization of the ancient dialectical art. He shows a tendency to create jargon but also a sense of humor in his coining of terms. His habit of capitalizing what he considers to be Important Words is a little disconcerting until one realizes that any abstraction really significant to him has the Real Presence of a proper noun.

All in all, Braybrooke is convincing enough in arguing that need fulfillment could indeed be programmed on the agendas of modern democracies, although he does little to dispell the likelihood that the concept of preference will win out in any foreseeable competition between the two concepts. The book is clearly written and can be recommended to general readers at the undergraduate level and beyond.

HENRY A. MYERS

James Madison University

Slightly Beyond Skepticism: Social Science and the Search for Morality. By Leonard W. Doob. New Haven: Yale University Press, 1987. 319p. $25.00.

Leonard Doob's book is not a work in political or moral philosophy, but its ambition is to serve both philosophy and the social sciences by providing a continuous critique and reconsideration of many themes that he identifies as central to the purpose of articulating and refining a theory of moral actionl Doob identifies a series of questions—eight in all—that he claims are implicitly, if not explicitly, raised every time a person makes a moral judgment concerning another person. The extent to which people fail to address all eight questions in taking a theory of moral action. Doob identifies a cates to him the extent to which there is a slippage between the ideal of moral action and the practice of all too human actors. Doob relies upon an encyclopedic knowledge of the literatures of both the social sciences and analytic philosophy in order to advance his argument.

Doob's basic structure for comprehending the varying importance of his eight questions, in which there is an ever-shifting terrain of principles, observers, and groups, coupled with his desire continually to frame skeptical and qualifying statements regarding the purchase of various propositions that he puts forward as governing moral behavior results in a book that is very comprehensive in scope and intricate in detail. To take but one instance, in chapter 11 he addresses the question of the role that utopias might play for advancing moral reasoning. He introduces the issue at this point because it seems to follow from a series of considerations having to do with the distinctions between dreams and reality in determining the facticity of concepts crucial to a human capability to be moral. Such slippery concepts as truth and predictability, justice, beauty, and perfection all are enmeshed in Doob's considerations. Yet all of these relate back to the skeptical thoughts with which he launches his study, namely, that "perfect knowledge and guides to morally complete judgments and actions are unattainable but approachable" (p. 8).

It is impossible to summarize the findings presented in this book. In part, this is due to the ambition and scope of Doob's project. But it is also impossible because the very structure of the book is tentative, composed of details that are not demonstrated to be of immediate or clear relationship to other details in the development of an argument. Throughout the book, there are propositions, skeptical notes regarding propositions, references to other sections of the book for further argumentation, meandering, and requalification of arguments that challenge some of the skeptical comments. Because Doob seems every bit as concerned that his reader understand the processes that went into his formulation of his argument, the argument that he seeks to advance gets lost. God may very well live in the details of a work, but God dies by detail as well.

One might very well accept the message of Doob's book—that one must always be both skeptical about the certainty of one's knowledge and also act morally by considering as many of the factors that he claims go into making such decisions—and yet have little or no use for the way in which he suggests that readers consider his approach to the study of morality. Early on in the book, Doob straightforwardly confesses that his approach might not be for everyone: "The schema works for me; it enables me to utilize tidbits of profundities in a systematic manner. I can only hope that the reader will have a similar experience" (p. 16). In his admission, the courage of Doob in writing such a book is clearly evident. Unfortunately, the experience of reading the book leads one to suspect that talking with Doob about the limits and powers of moral reasoning might be the more fruitful course.

THOMAS L. DUMM

Amherst College

Constructing the Political Spectacle. By Murray Edelman. Chicago: University of Chicago Press, 1987. 137p. $19.95 cloth, $7.95 paper.

The well-known author of *Political Language* and *The Symbolic Uses of Politics* continues his examination and critique of modern democratic capitalism by dissecting (debunking?) what he terms the "political spectacle." Building on the work of Nelson Goodman, Michel Foucault, and Jacques Derrida, he provides a devastating portrait of modern-day democratic politics—one that verges, at times, on the cynical—but a cynicism not of Frank Kent nor H. L. Mencken but rather of the subdued but still hopeful Left. The lofty disengagement of Lippmann is nowhere to be found.

For Edelman, present-day politics is largely sham, artifacts, symbols, mystifications, and fetishes. In successive chapters he describes and explains how social "problems" are "constructed"; how leaders are manufactured and the uses they, in turn, make of their position; how political enemies are likewise created and used; and how news of politics is manipulated. In all these analyses, Edelman sees ideology at work constantly interpreting events, persons and political debate. Ultimately, ideology or the logical constructions of the political spectacle dominate and shape real forces and interests. Edelman even admits that his relativist view of our symbolic life is but another ideology. Nevertheless, he contends that all ideologies are not equal; that some provide more accurate depictions of "reality" than do others. He does not explain why nor provide a decisive test by which to reject the false or partially false. But why do we have ideologies? The answer it seems is found in the nature of an uncertain environment and the psychological needs of people to understand their lives and, not least, as tools or tactics to be employed in winning political struggles.

Much of what the author claims will make eminent sense to social scientists, generally and even to adherents of the rational mode of analysis and its current embodiment in public choice. Like Edelman, many other scholars have been "constructed" and "deconstructed" by their teachers and the ideological battles of their times. Those who have been "deconstructed" by the likes of Schumpeter, Buchanan, Tullock, Downs, Olson, and Riker are apt to view this book with some ambivalence. On the one hand Edelman appears to treat politics as a rational pursuit of interests and values while on the other he comes closer to, say, Pareto and all those who view the more visible aspects as a vast con game in which the "rational" ones exploit the helpless by verbal and other symbolic manipulations. Some of this latter approach stresses the less rational elements of life.

Public choice or, at least, the Virginian version, apparently views politics in somewhat the same way, seeing self-interests and rationality as the bedrocks of positive analysis but then showing how these two assumptions produce divergent outcomes in the market and polity with the latter being shown to be demonstrably less efficient in allocating scarce resources and frequently unfair in its distributive choices. Edelman is not much concerned with allocative efficiency; he is mightily concerned with deprivation, exploitation, anomie, and inequality. His villains are obvious from the illustrations he selects, all of which are common staple among left-of-center thinkers. Public choice would substitute another set mostly from among liberal and populist politicians, bureaucrats, and interest groups. But more importantly they would focus not on the capitalist economy as the cause of social ills as on the political rules of the game or institutions that suit the purposes not of profit makers but of political groups and rent seekers, many of whom are not capitalists. Indeed, in public choice it is a capitalist economy that suffers from the fetishes of populist democracy. Accordingly, constitutional reform offers the only escape from systematic irresponsible political action. Believing as he does that "for most of the human race political history has been a record of the triumph of mystification over strategies to maximize well-being," Edelman offers mostly a counsel of despair. He does not consider the possibilities of simply reducing the role of politics in everyday life.

Regardless of obvious differences in analysis and ideals, Edelman's book should be read by all, including public choice scholars, for it depicts in its 130 pages of often passionate and moving words and ideas the essential phoniness of politics. He reminds us that preferences are rarely fixed except in our positive models and that as interpretations and rationalizations ideologies are of extraordinary importance in political life. We are indebted to Edelman for a challenging philosophical essay.

WILLIAM C. MITCHELL

University of Oregon

The Philosophy and Politics of Freedom. By Richard E. Flathman. Chicago: University of Chicago Press, 1987. 360p. $42.50 cloth, $16.95 paper.

This book audaciously restructures the longstanding philosophical debate about freedom. It is built around a brilliant scheme for classifying "kinds" of freedom—a scheme that deserves to replace the influential "positive" and "negative" terminology of Isaiah Berlin and others.

Flathman distinguishes five kinds of freedom (each of which corresponds to a distinctive kind of "unfreedom"). Freedom$_1$ is the freedom to perform "self-activated movement." But this freedom is obviously too limited to distinguish the cases that would hold significance for political theory. Freedom in this sense is fully satisfied by the example with which the book begins: "A baby emerges from the womb. Nothing apart from itself is moving it, but it moves nevertheless. . . . Some of the baby's movements go unimpeded: It extends its legs, twists and turns" (p. 15).

While this baby fully satisfies Freedom$_1$, it has no self-consciousness about what it is trying to do. Even when its movements are unimpeded, it has freedom only in a very thin sense.

Freedom$_2$ is the freedom to perform "intentional or purposive action." But actions may be intentional while also lacking in significant self-control or autonomy. For example, purely habitual actions satisfy Freedom$_2$. Flathman describes the smoking of Able, a member of his car pool: "Her deadly instrument moves irregularly from hand-to-hand and hand-to-mouth, she puffs rapidly and then slowly, exhales first in my direction and then in that of another sufferer, she flicks her ashes sometimes on the floor, sometimes out the window, occasionally in the ashtray. These depressing sequences, moreover, are learned and practiced. Able knows how and how not to smoke. She conceives of herself as smoking in a sophisticated manner" (p. 97).

Able "may say that she 'can't help' smoking, or that it is 'impossible' for her to stop smoking" (p. 99). Her *akrateia*, or weakness of will, shows that while she has fulfilled Freedom$_2$, she has not fulfilled Freedom$_3$.

Freedom$_3$ is autonomy. It is the freedom to perform actions "in the pursuit of a self-critically chosen plan or project that the agent has reason to believe is consonant with defensible norms or principles" (p. 322). But such a project, reflecting an individual's self-reflective choice, may be incompatible with the social constraints of the community. The individual, viewed in isolation, may seem free but she may, nevertheless, be significantly constrained. Consider, for example, Able's problems with her colleagues: "Able aspires to good relationships with her academic colleagues but frequently publishes sharply critical analyses of their scholarly work. Some at least of Able's colleagues resent this practice and are cool to her because of it. Charlotte and Dorothy attribute Able's conduct to jealousy and personal insecurity and urge Able to seek counseling so as to better understand and control her inclinations. They argue that following this course would enhance Able's freedom" (p. 104).

While Flathman is critical of this attempt to connect community and freedom, it does express an identifiable position, one that he calls Freedom$_4$, or communal freedom. It is the freedom to perform actions that conform to principles "that are authoritative in the agent's community." Able's misfortune is that she would like to live in a scholarly community defined by vigorous mutual criticism. In fact, she happens to live in one whose highest principle is collegiality. In pursuing her notion of the first she violates the second and it is the latter that happens to be authoritative in her community.

But Freedom$_4$ could be fulfilled without the authoritative character of the communal principles having any connection to a self-critical choice by the agent. Consider Able's Lutheranism: "She was confirmed in the church at the age of twelve, having paid scant attention in confirmation classes and has given little or no thought to such matters since. . . . As a person of religious faith, she accepts that it is appropriate for her to attend church. But if we ask her why, if we ask for a more detailed and at least in that sense a more satisfying explanation for her more or less regular attendance at services, she can make no (further) response" (pp. 189-90).

Able's Lutheranism lacks what Flathman calls Freedom$_5$ or "fully virtuous freedom." It may conform to principles that are authoritative within a given (religious) community, but it is not "self-critically chosen." There is a sense in which she is constrained by her ingrained irrationality to be unfree.

Despite its attractions, Freedom$_5$ does not define a principle of freedom Flathman recommends to us. After meticulously developing this five-part framework, he opts for Freedom$_2$ as the most important and defensible conception. The rival theories confuse freedom with virtue (Freedom$_3$), or with community (Freedom$_4$) or with both (Freedom$_5$). In taking this sharp turn in favor of what has been called negative freedom, it is arguable that Flathman renders himself vulnerable to objections that the more positive positions (Freedoms$_{3-5}$) could be invoked to rebut. There are serious questions about whether his position can handle the challenge to freedom posed by indoctrination, preference manipulation and collective brainwashing. While Flathman mentions these (p. 224) it is entirely in the context of coercion (or a history of coercion) that could be objected to on Freedom$_2$, or negative liberty grounds. However, more subtle forms of collective indoctrination may significantly interfere with our liberty without advocates of Freedom$_2$ having any basis for an objection. For example, suppose the controversial thesis C. E. Lindblom put forward in his "circularity" argument were true. Whether or not Lindblom was right about its empirical application to U.S. society, it describes a possible pattern of collective indoctrination (in favor of big business) that would severely undermine the sense in which citizens enjoying Freedom$_2$ were free in their processes of volition formation. Such citizens would be subject to collective political manipulation without the kind of naked coercion that would give Flathman a basis for objecting on grounds of Freedom$_2$.

Negative freedom, even in the sophisticated theory developed by Flathman, is not enough. But even if I am right in this criticism, this book deserves to transform the debate about liberty by placing one of the most contentious issues in political philosophy within a new and compelling theoretical framework.

JAMES S. FISHKIN

Center for Advanced Study
in the Behavioral Sciences

Interpreting Politics. Edited by Michael T. Gibbons. Essays by Peter Winch, Steven Lukes, Brian Fay, Charles Taylor, Clifford Geertz, William Connolly, Jürgen Habermas, Hubert Dreyfus, Michel Foucault, and Richard Rorty. New York University Press, 1987. 264p. $30.00 cloth, $15.00 paper.

In the opening book of the *Politics*, Aristotle strove valiantly to distinguish politics (in particular) from social phenomena (in general); and while we may doubt that politics necessarily means the art of ruling and being ruled in turn by peers and equals, we can at least appreciate the effort to keep the polis from dissolving into household economy, or the family, or mastery and slavery. The obvious problem of *Interpreting Politics* is that politics (however one delimits it) is interpreted by so few of the distinguished writers assembled here. Drawing together figures as eminent as Winch, Lukes, Taylor, Geertz, Connolly, Habermas, Foucault, and Rorty can do nothing to disguise the fact that this collection should have been called *Interpreting (Many Diverse) Social Phenomena*. For the justly celebrated essays of Winch, Lukes and Geertz (this last particularly striking) are really concerned with the ethics and methods of anthropological investigation, while the fine performances of Habermas and Foucault deal principally with Gadamer's hermeneutics and Nietzschean genealogy respectively. It is not just that some (particular, favored) notion of politics is absent from this collection. In two-thirds of the book no notion of politics is "there" at all—not the polis as enlarged *psyche* (or contracted *kosmos*), not Leviathan as an antidote to fear, not eternal republican peace as an approximation to the kingdom of ends, not the state as an epiphenomenon whose ghostly half-life finally withers. The volume offers more on the general concept of "interpretation" (Habermas' "The Hermeneutic Claim to Universality," Foucault's "Nietzsche, Genealogy, History") than on the "object" supposedly being interpreted. Indeed, a more accurate title might be *The Politics of Interpretation*.

Only three of the essays in this collection, in fact—Taylor's "Language and Human Nature," Rorty's "Method, Social Science, and Social Hope," and Connolly's "Appearance and Reality in Politics"—offer an "interpretation" of politics, a reading of politics, a political *theory* that flows unforcedly out of a more general understanding of the world (so that politics is both delimited, bounded, à la Aristotle and yet related to larger concerns—so that, in Oakeshott's phrase, "our political ideas and what may be called the rest of our ideas" are "text and context" in "a single world of ideas"). Taylor, Rorty and Connolly achieve this by wholly different means; but they do, at least, "interpret politics."

Taylor's method is to derive, at the last possible moment, rather radical political conclusions from what had looked like innocent (if dazzling) intellectual history. Most of his essay offers an account of the meaning and function of "language" since antiquity. For Plato and Augustine, he urges, the *kosmos* was the expression of the Ideas of God (the Word realized). The seventeenth-century Cartesian-Hobbesian scientific revolution banished the notion of expression and made words (nominalistically supplanting the Word) merely "designate" objects. The romantic revolution against Enlightenment "objectivity" resuscitated language as expression—but now human expression in art that supplants religion, not the world as expression of divine Idea. From this effective *tour d'horizon* (argued brilliantly in detail), Taylor suddenly extracts a drastic political consequence: "As a civilization we live with a compromise. In our scientific understanding, we tend to be men of the Enlightenment, and we accept growth, productivity and welfare [as being] of fundamental importance. . . . The main limits of public policy are set by the requirements of production [which are in turn] set by scientific means. . . . But private experience must be given its expressive fulfillment. There is a 'Romantik' of private life, which is meant to fit into a smoothly running consumer society. However effective this compromise may be politically, it is a rotten one intellectually; it combines the crassest scientism (objectivism) with the most subjectivist forms of expression" (pp. 157ff.).

Much more radical from the outset—not as a surprise sprung at the end—is Connolly's effort to rehabilitate a refined "false consciousness" (as an antidote to Winchian-Wittgensteinian "self-understanding"). Blue-collar workers formulate and cling to the notion of "voluntary sacrifice" for the future good of their families, deriving their sense of dignity and worth not from work but from those families' *recognition* of sacrifice. Evidence of the unsatisfactory nature of their labor does not lead them to identify themselves with other social victims (whom they despise as shiftless), does not lead them to radical politics. This is intelligent Hegelo-Marxism, applied to an important matter.

In contrast to Taylor and Connolly, Rorty is (without being merely complacent) much better satisfied by modern Western achievements. "Human solidarity" is very important, but "bourgeois liberalism" seems to him "the best example of this solidarity we have yet achieved, and Deweyan pragmatism the best articulation of it" (p. 231). And that "articulation" (for Rorty) matters, since it is Dewey who best reveals that the disappearance (or rather never-existence) of a Kantian "transcendental subject" need not make liberalism vanish as well—that we need (or can have) a transcendental subject in moral-political thought as little as we need (or can have) nature's own voice in science. The search for truth and foundations must yield to the modest notion of "having a better idea."

One need not agree with Taylor's, Connolly's or Rorty's interpretation of politics—arguably, we would be better served by a kind of left Kantianism that fused the very "foundations" that Rorty spurns with some of Connolly's dissatisfaction—to see that these three at least have an interpretation of politics. In their hands politics may (unavoidably) shade into other social phenomena, but it does not dissolve.

Given the fact that this collection ranges from cultural anthropology to hermeneutics to linguistics—with a trace of politics—Gibbons has (in his introduction) succeeded in making these writers speak to each other more than one would have thought likely. But someone ought to have caught the unconscionable number of typographical errors that mar the volume.

PATRICK RILEY

University of Wisconsin

The Decline of Urban Politics: Political Theory and the Crisis of the Local State. By Mark Gottdiener. Beverly Hills: Sage, 1987. 299p. $29.95 cloth, $14.95 paper.

This is the kind of book that will be extremely useful to graduate students preparing for their comprehensive examinations. It summarizes clearly and synoptically an enormous literature dealing directly or indirectly with the structure and function of urban politics as it operates within the larger political-social system. We have, for example, substantial discussions of "state-centered" and "society-centered" approaches to local government (Ostrom, Peterson, Dahl, Pahl, Harvey, Castells); a systematic treatment of structuralist-Marxist approaches to the state; an account of Foucault's notions of power and surveillance in the modern world—all this in addition to a lengthy and seemingly comprehensive description of the more standard political science literature on urban politics. To my mind, the discussions are accurate, intelligent, and well-integrated.

But Gottdiener's ambitions go far beyond a literature review. His goal, it seems, is to provide a systematic theoretical account of the role of "the local state" in late twentieth-century capitalism, thereby providing a basis for understanding the processes of power and decision making in urban politics. The degree to which this goal is achieved remains, as far as I can tell, an open question.

The fundamental problematic is outlined in the book's title. A variety of factors—low levels of political participation, jurisdictional fragmentation, the influence of national governments—have rendered urban politics moribund. Increasingly, the local state is little more than an agent of the larger state and, as such, performs essentially a social control function involving "massive surveillance, control of space, police force, imprisonment, and the regulation of use-values and needs" (p. 274). Society-centered and state-centered theories of urban politics either ignore this fact or fail to provide compelling, comprehensive accounts of why and how it occurs.

Gottdiener's disposition throughout is to deal with seemingly opposed or dichotomous viewpoints by incorporating them into a broader, more inclusive perspective. Thus, for example, society-centered and state-centered theories all contain a good deal of truth; any satisfying approach, therefore, must include a variety of analyses that have hitherto appeared to be contradictory. The central formulation in this respect is what Gottdiener calls a "two-tiered conception of the state." The first tier, roughly analogous to the Marxian material substructure, includes "those laws or State edicts establishing the entitlements and liabilities of capitalist private property"; the secondary involves the "struggle among social groups . . . over the division of wealth" (pp. 168–69). By incorporating a political element into the first tier and, by implication, an economic element into the second, Gottdiener would claim to overcome the seeming opposition between vulgar, economic-deterministic Marxism and the structuralist Marxism of, say, Poulantzas.

It seems to me that here and elsewhere, Gottdiener may be overly optimistic about the degree to which such oppositions can be reconciled. For example, if the first tier involves the politically generated definition of property entitlements, there are surely Marxists who would still want to know where that definition comes from and who would seek to explain it straightforwardly in base-superstructure terms with the mode of production given causal priority in—so to speak—the last analysis.

But beyond this, it is not entirely clear exactly how fruitful Gottdiener's approach is for understanding urban politics. Among other things, we encounter the following conclusion: "The theory of the local State presented here explains public policy as the complex interplay of social interests manifested through the forms of the State" (p. 216). This seems a rather meager payoff. The state and its forms do have a certain and important degree of autonomous impact upon policy; but then so does the class structure of society and the various economic interests that comprise the social formation. All of this seems unremarkable enough. And exactly how these various features are interrelated such that one might develop a comprehensive and explicit *theory* of the policy process remains unclear to me.

It may, nonetheless, be clearer to other readers of this book. If nothing else, the effort to situate the analysis of local politics systematically within the larger domain of state theory is an important one, and Gottdiener has made a useful, informed, and interesting contribution to that effort.

PETER J. STEINBERGER

Reed College

The Nature of Politics: Selected Essays of Bertrand de Jouvenel. Edited and with an introduction by Dennis Hale and Marc Landy. New York: Schocken Books, 1987. 254p. $28.50.

Bertrand de Jouvenel died in March 1987 at his home in Paris at the age of 84. Political theorists are most familiar with his books *Power* (1945) and *Sovereignty* (1957). The present volume makes available some of his most important essays.

Jouvenel was bilingual, and his English was as elegant as his French. The essays collected here are at once a delight to read and an important substantive contribution to political science.

Insofar as was humanly possible, Jouvenel aspired to take a disinterested approach, beyond ideology. He abhorred fanaticism of whatever variety. He experienced deeply and at first hand the horrors of totalitarianism, and he sought to lay bare the intellectual and moral roots of this tragic phenomenon. Condemning the terrible simplifications of ideological sloganeering, he reappropriated for political science the Platonic discovery that institutions do not grow out of wood and rock but are magnifications of the personal psychologies of those who make them work. Justice for him was not something abstract but a product of human will and moral choice. He deplored the increasing tendency in political thinking toward the "moral emancipation" of public authority. Preoccupation with the legitimate *origins* of power should not crowd out concern with power's legitimate *use*, he insisted.

Bertrand de Jouvenel taught that politics has a nature more fully realized in some societies than in others. Politics is concerned with the building of aggregates of human beings. In this connection, he made a cardinal distinction between the role of the *dux* and the role of the *rex*. The *dux* is the person who "leads into action a stream of wills." Every progressive society leaves the way clear for innumerable *duces* to call into being "action groups of all sizes and natures." These leaders are the social magnets around whom human beings coalesce for purposeful action. Inevitably, these groups come into conflict with each other and "multiply frictions and tensions." These frictions and tensions call forth the need for a second figure, the *rex*, who is concerned more with regulation than with initiation. Government plays the role of the *rex*, who is regulator, rectifier, stabilizer, and adjuster of competing claims.

Politics for Jouvenel is much more than government. Too much government regulation can kill politics, he held. Thus he helped prepare the way for a renewal of the Aristotelian understanding that politics is coeval with the entire social existence of human beings.

Jouvenel held that society was "an unceasing flow of authoritative initiatives," each of which "comports a moral responsibility" and in which may be found more forms of authority than can be found in government. Government is legally the highest form of authority, but morally it is the lowest because it depends on the force of intimidation.

Jouvenel's work compares favorably with that of Hannah Arendt, Carl J. Friedrich, and Michael Oakeshott. More than any of them, however, his work is animated by a feel for daily political practice. Jouvenel's background as a political journalist in the 1930s serves him well, for he writes simply and intelligibly for the serious reader and informed citizen. He was the most unacademic of professors of political science.

A mere glance at some of the titles of the essays in this volume is sufficient to show that every political scientist regardless of field will find something of value here. They range from the more theoretical ("The Nature of Politics," "Authority: the Efficient Imperative," and "On the Nature of Political Science") to the more practical ("Political Science and Prevision," "Thoughts on a Theory of Political Enterprise," "The Manners of Politics," and "The Team against the Committee").

The volume contains an eloquent Preface by Wilson Carey McWilliams, a useful introduction by the editors, and a selected bibliography of Jouvenel's writings from 1928 to 1985. It also contains a helpful index.

DANTE GERMINO

University of Virginia

The Thinking Revolutionary: Principle and Practice in the New Republic. By Ralph Lerner. Ithaca: Cornell University Press, 1987. 238p. $24.95.

This striking and very stimulating book argues a thesis aptly described by the title, but one quickly observes that the decided emphasis falls on the adjective *thinking*. Lerner aims to persuade his readers that some of the U.S. founders were given to critical, rigorous thinking about their political views and choices, and he amply furnishes the evidence that supports this claim, combing *The Federalist*, the writings of Adams, Franklin, Jefferson, early judges of the Supreme Court, and a variety of men influential in early formulation of policy toward the Indians. He has uncovered a lode of argument, debate and reflection that richly manifests the signs of thinking: recognition of complexity, deliberative weighing of competing goods, attention to the complementary functions of rhetoric and ideas, appreciation of moral intricacy, capacity for critical scrutiny of one's own endeavors, resistance to the confusion of politics or revolution with religion.

Lerner's exceedingly careful, nuanced presentation of the founders' thinking requires readers to reflect on two major points. First, his work challenges the "scholars of the ideological school" (p. 9) who have recently dominated studies of the U.S. founding. That school (Bailyn, Wood, Pocock, and others) has searched assiduously for an ideology controlling the minds of the founders, believing it can thereby get closer to the socioeconomic realities behind the deceptive role of formal discourse in public life. Lerner argues against this very influential approach by attempting to show that it does not get history entirely right. He leads his reader to consideration of important instances where self-conscious reflection and freedom of mind are at work, relatively free of unrecognized biases. Some among the founders "took special pains to distance themselves from much of what everyone else opined" (p. 34). Some "took thinking clearly to be their paramount duty," especially in the sense of thinking through "with care and deliberation" how their truths "would work in an actual world," believing and acting on "the premise that the highest activity of the thinking revolutionary is constitution making and constitution preserving" (pp. 34–36). This thinking by the revolutionaries was less ideological than practical, very attentive to circumstances, aware of moral and political principle but also of the constraints on following principle. Lerner makes his case primarily by presenting compelling examples of thoughtfulness, but to clarify his sense of the founders as thinkers, he undertakes at one point to state his method for interpreting their writings and to compare it with that of the ideological school; no reader of this book should fail to examine his thesis about interpretation at the conclusion of the prologue (pp. 30–38).

Lerner's second major theme is the dilemma of a society built on the liberated pursuit of self-interest. Lerner's founders saw the limitations inherent in this Lockian vision and the inadequate provision made for overcoming its weaknesses. But does he think they saw far enough? It is no accident that the core of this book is framed by chapters on Ben Franklin and Tocqueville. "Franklin, Spectator" examines the most beguiling early U.S. statement of democratic man's self-understanding. Franklin's story tells, in a manner both urbane and charming, of his pursuit of interest through methodical self-improvement, as Lerner shows in a graceful analysis. Tocqueville later saw something similar to this outlook (albeit without Franklin's urbanity and charm) as the true soul of Yankee man, and thought it was destined to be the dominant U.S. type and eventually the future for all mankind. Lerner's concluding chapters explore the "burden of color" and Tocqueville's study of the "three races that inhibit the territory of the United States." He suggests, again in a persuasive, nuanced way, that Tocqueville, guided by his teachers Montesquieu and Rousseau, may well have seen the full range of dangers attending this outlook more adequately than any U.S. thinker.

DONALD J. MALETZ

University of Oklahoma

The Languages of Political Theory in Early Modern Europe. Edited by Anthony Pagden. Cambridge: Cambridge University Press, 1987. 360p. $42.50.

Part of a developing series of methodologically kindred texts, Ideas in Context, this volume of essays is influenced by recent "linguistic turns" in literary criticisms and philosophy. The context in question is political languages: modes of discourse about political life characterized by distinguishable vocabularies, rhetorical strategems and permissable theoretical "moves." This approach, as set forth by Pagden in his introduction and the essay by Pocock, eschews the deciphering of isolated texts and an exclusive regard for "who influenced whom," as well as the supposition that texts can be radically severed from conscious intention (even if that intention differs from what is achieved by a text: "History consists largely of unintended performances" [p. 31]). This perspective entails the idea that "the history of political thought is now more accurately described as the history of political discourse" (p. 19).

The linguistic context of political discourse may be an historical language, such as Hebrew, Greek, and Latin. But Pocock primarily means (given the difficulty of access to oral "text") written idioms, "language games," institutionalized in the practices of dominant professional communities, such as the languages of lawyers, the clergy, and the court. Changes occur in such languages either as a result of the "politics" of the language itself, of a need to conserve the privilege of the dominant communities against competitors, or as a result of the diffusion of the idiom to other groups in the wider society, who may understand its term or apply its strategies in a way different from its originators. Discovering the various languages employed in a given text is one task. Central also to the enterprise is the description of the dialectical relationship between individual linguistic practitioners or between text and its linguistic context.

Part 1 focuses on the language of Aristotelean or Thomistic political thought. The essays in this section include Rubenstein's description of the medieval and early modern identification of *politicus* with *republican*. Kelley portrays the debate among the jurists regarding the latitude to be given to the interpretation of the lawgiver's intention. Pagden's contribution focuses upon Spanish Dominican attempts to apply critically the propositions of natural law reasoning to the legitimacy of confiscation of Indian land by the King of Spain. Tuck shows that one of the central aims of modern natural law theory was the refutation of ethical scepticism.

The second set of essays investigates the language of classical early modern republicanism. Skinner views More's *Utopia* as a serious (nonironic) criticism of property as incompatible with humanist republican notions of the best regime. Viroli attempts to reconcile Rousseau's general will with modern negative liberty. Mulier describes the way in which Dutch political thinkers used the Venetian republic as a model of the mixed constitution. Goldie demonstrates the congregationalist flavor of Harrington's reformed Christian civil religion.

The language of commercial society, of "private vices, public benefits" is the theme of Part

3. Goldsmith opposes Mandeville's unabashed defense of commercial self-interest to the dominant code of "civil humanism." How Pufendorf revised Hobbes's notion of the state of nature is the key to understanding why Adam Smith views commercial society as the fourth stage of history, according to Hont. Bellamy analyzes Antonio Genovesi's attempt to reconcile Christian virtues and commercial activity within the framework of a Newtonian view of man and nature.

Part 4 explores the language of the scientific study of society. Rossini, despite her characterization of the latter Hobbes as "scientific" (and hence as devaluing prudence), sees a basic continuity between the younger Hobbes's humanistic interest in Thucydidean history and the role afforded to history in his mature writings. Pursuing certain suggestions by Foucault, Wokler concludes that Saint-Simon's notion of a science of organization grounded in physiology ended the Enlightenment project of a moral, political science of man. Shklar, reflecting on Hamilton's observation on elections in the *Federalist Papers*, notes the need of a representative democracy for a systematic study of voters' desires and the way in which that science of voting enhances the public's perception of the importance of the very elections that give rise to that science.

Although some of the essays (notably those by Rubenstein, Kelley, Goldsmith, and Shklar) emphatically fulfill the promise of the editor's theoretical perspective, that framework hardly seems Procrustean for the remainder, especially those dealing with individual theorists. That does not detract from the interest they will have for the student of political philosophy. Bellamy's description of Genovesi, for example, reveals a fascinating thinker. Pagden's own contribution does not discuss in any detail the transformation of Thomas's political theory by the School of Salamania. Yet the essay demonstrates not only the fertility of scholastic reasoning with regard to property and to war (perennial questions indeed) but also the consistency, honesty, and courage of Vitoria.

In short, this volume sets forth an interesting, controversial view of the nature of political philosophical inquiry. Readers, for the most part, need be neither fluent in the idiom of the volume nor in agreement with its general intention to find most of these essays instructive.

WILLIAM BAUMGARTH

Fordham University

Belle Moskowitz: Feminine Politics and the Exercise of Power in the Age of Alfred E. Smith. By Elisabeth Israels Perry. New York: Oxford University Press, 1987. 280p. $24.95.

Partner and I: Molly Dewson, Feminism, and New Deal Politics. By Susan Ware. New Haven: Yale University Press, 1987. 327p. $25.00.

The Grounding of Modern Feminism. By Nancy F. Cott. New Haven: Yale University Press, 1987. 372p. $29.95.

One of the popular misconceptions of women's history is that feminism failed because after women got the vote, they failed to use it. Several prominent historians once included that axiom in their analyses, and political scientists (myself included) took their conclusions for granted. New research by feminist historians is beginning to disclose just the opposite; women did have an important impact on the political realm in the twenties and the thirties. The problem was that earlier researchers looked for the wrong things and looked in the wrong places.

Two excellent biographies and one insightful general history illuminate some of the many roles women played. They were active both in and outside of the major political parties; they lobbied for specifically feminist issues; agitated for reform of women's health, morals, and working conditions; and actively supported important (male) politicians. Some even ran for office. Several won.

Belle Moskowitz's contribution was as Al Smith's campaign manager in his 1928 presidential campaign and the author of many of his progressive social programs in the preceding decade. A woman who knew her place and felt she could be more effective working quietly behind the scenes; she never had the titles or official positions her power entitled her to. Officially she was only Smith's publicist. Unofficially, anyone who wanted to influence Smith or his campaign had to go through her. According to Moskowitz' biographer, a professional historian who is also her granddaughter, he recognized Moskowitz' importance by offering her jobs and recognition that she refused. Indeed Moskowitz was so self-effacing that she never allowed herself to be quoted in the newspapers and kept few records of her own in the belief that they weren't important. By utilizing Al Smith's extensive papers and other archival sources the author pieced together her life and documented the importance of someone whom the *New York Times* called the most powerful woman in U.S. politics in a front-page obituary.

Moskowitz' life and career is a case study of how a traditional wife and mother was caught up in a cause much larger than herself and through it transcended her limited ambitions. She became involved in settlement work and continued it after marriage and motherhood consumed most of her time. Early widowhood forced her to earn her living as a writer, an industrial mediator and a publicist and brought her into contact with Smith, whose inner circle included many other progressives. She retained her position, without portfolio, after her remarriage, while her influence increased less through her own design than through the valuable services and valued ideas she brought to Smith. Her death in 1933 was almost timely, in that it followed Smith's own decline from power.

Molly Dewson was only a few years younger than Moskowitz and was also involved in progressive causes. But unlike Moskowitz her style was more feminist than feminine and she did accept a position as head of the women's division of the Democratic National Committee in 1933. A disciple of social reformer Florence Kelley, her particular concern was minimum wage laws for women. Her work on this and other protective labor laws brought her to Eleanor Roosevelt's attention in the twenties. ER recruited Dewson to work in the women's division of Smith's 1928 campaign. The two became close, and Dewson was put in charge of organizing women for Roosevelt thereafter. Her successes in this arena gave her considerable influence, which she used to put women like herself into key positions in the Roosevelt administration. Frances Perkins was her protégé, but Dewson was responsible for at least a hundred other female appointments, and these women in turn brought in others under them. Dewson also influenced campaign practices, inventing the "rainbow flyers," which became a staple of Roosevelt's campaigns, and organizing a cadre of 15 thousand New Deal saleswomen. Even Jim Farley called her "the General."

Cott's book focuses on the less recognized but no less important hundreds, even thousands, of dedicated women who worked on "women's issues" at the national, state, and local level during the twenties and thirties. Most did not call themselves feminists after 1921. This label was claimed by adherents of the National Women's Party (NWP), whose primary goal was attainment of legal equality for women, preferably through passage of the Equal Rights Amendment. But all were influenced by the ideology of feminism, which emerged about 1910 and reflected a different attitude than that of the nineteenth century's women's movement. Some of them worked through a coalition of women's organizations called the Women's Joint Congressional Committee (WJCC). It claimed responsibility for passage of the Mothers and Infants Protection Act, the first national social welfare law, and for the Child Labor Amendment. Both the WJCC and NWP took credit for several laws that freed women's nationality from their husbands', thus allowing U.S. women to retain their citizenship on marriage to aliens.

In the states women fought each other over protective labor laws for women only, particularly those limiting the hours women could work, prohibiting night work, and excluding them from some occupations. These were urged by progressives to reduce their exploitation by industrial capitalism and opposed by the NWP as a limit on women's ability to support themselves. Less dramatically, women slowly pushed into professional jobs and fought retrenchment by laws and practices that tried to remove married women from the labor force on the grounds that men needed the jobs to support their families. Women also organized for peace and disarmament and, along with similar groups, were tarred by the Red Scare, which tried to link feminism and socialism. Throughout the decade between the world wars, women, organized and otherwise, tried to resolve the tension between a consciousness of women as a group and a desire to join men as equals. That tension provided the ground in which the modern feminist movement grew.

Jo Freeman

Brooklyn, New York

Knowledge and Class: A Marxian Critique of Political Economy. Stephen A. Resnick and Richard D. Wolff. Chicago: University of Chicago Press, 1987. 352p. $32.50.

I suppose that there are two conceptions of what makes a theory Marxist. In one conception a Marxist theory advances characteristic claims about the importance of technology and property in the historical development of social life and is associated with egalitarian values intended to inform political practice. In so far as Marx used an intelligibly special method, that method is held to be bad.

Resnick and Wolff offer a distinguished contribution to a quite different conception of Marxist theory. To be Marxist—or rather, Marxian—is in their view to adopt a particular methodological stance, "a strictly nonessentialist ontological formulation," which neither collapses the real into the idea of the real, like bourgeois rationalist essentialism, nor collapses the idea of the real into the real, like bourgeois empiricist essentialism (p. 49). Freed from these essentialist fallacies of mainstream Western philosophy, Marxian theory is bound to regard the social and natural worlds as existing in a continuous flux—or process—irreparably connected to the flux of our thoughts about the social and natural worlds. There are no entities in the world sufficiently distinct that one could speak of them as elements subsequently entering relations with other elements. There is no distinction between cause and effect and so no priority in the order of the real. There is no unpolemical distinction between science and ideology, truth and falsehood, and so no priority in the order of the idea of the real (pp. 4, 6, 56, 54). We are left to contemplate the overdetermination and mutual contradiction of each fluctuating aspect of the intellectual-real with every other fluctuating aspect in a complexly interconstituted social and natural whole. The great merit of Althusser was to make explicit this central insight of Marxian theory. But even Althusser did not go quite far enough, since he seems to have harbored a lingering suspicion that if Marxism were ever to renounce completely its commitment to the priority of the economic factor in the "last instance," all would be lost (p. 93). Resnick and Wolff are courageous enough not to rely on such nostalgia for the prehistory of Marxian theory and seek "to offer an alternative to *any* determinism" (p. 278).

Since they believe so consistently in the unordered interrelationship of all thoughts and things, the authors seem to face the problem of having to say something about everything before they are able to say anything about something. They tackle this problem rather adroitly by regarding nonessentialism on the one hand and the concept of social class on the other as the twin "entry points" to a theoretical practice whose end is the construction of an ever-more-complex specification of the real from an ever-more-complex specification of the concept of the real (p. 97). Rightly understood, *class* will eventually tell us all we can come to know about the social whole from the Marxian point of view. Whether or not we could gain an equivalent knowledge from building up an adequately complex concept of, let us say, *fetlock* is answered by the suggestion that the authors' practice is itself complexly overdetermined in their own life histories (p. 27). Perhaps they didn't ride horses in their early youth. Regarding the choice of *class*, they could therefore not do otherwise.

Given the radicalism of their methodology, the treatment of class—and then of enterprise and the state in the latter two-thirds of the book—is in many respects rather orthodox. "Fundamental" class relations are said to arise from the appropriation of surplus value, and "subsumed" class relations from its subsequent distribution. (The reader who has not properly digested the previous hundred pages may falsely think that this use of terms implies priority for the "fundamental" type of class relation [pp. 117-18]). There follow long and often interesting analyses of the relationship between the various sources and destinations of the surplus value that sustain the divers ranks of corporate finance, management, and state bureaucracy. (*Divers* is a word attractively deployed in the book.) Various equations are introduced to analyze the interrelationships among these kinds of revenue, interpreted as instances of contradiction among the various class positions constituted by the pattern of intersecting revenue flows. Since there is no one-to-one correspondence between positions and persons, the contradictions are not necessarily between social groups and may divide a person from him or herself (p. 158).

If one's epistemology allowed a judgment of theoretical priority among the numerous class processes that flow within and between the capitalist enterprise and the bourgeois state apparatus, one would say that Resnick and Wolff's theory was Poulantzian: among capitalists ownership is sidelined, and the power of surplus appropriation given center stage; among workers the productive laborers are singled out. Since the concept of surplus value is the base on which this superstructure of class distinctions arises, it is also a pity that the concept is not examined more closely in the light of recent work. The value equations are accounting identities, whose ability to express contradiction between classes reduces to the fact that if class A enjoys a part of given revenue, the same part is unavailable for class B. It is not clear whether accounting is by money or by labor time or at what level of aggregation it occurs, nor is the distribution between classes related to distributions of property ownership, to technology, or to the process leading to the outcome, given life by a theory of action. There is no discussion in the latter part of the book of the relation between value and price, of general equilibrium theory, of political values and the justice debate, of the recent switch from domination theories of class to exploitation theories of class or in the earlier part of the book of functional explanation as a non-ontological solution to the problem of economic determinism. In short, there is no discussion of Cohen, Roemer, Elster, or the more recent non-Althusserian incarnation of Eric Olin Wright. This is a measure of how far apart the two practices of Marxist theory have become.

On a late page Resnick and Wolff helpfully define a book as "a moment within an ever-changing relation of communication among people caught up and shaped within an ever-changing social totality" (p. 275). I was certainly surprised by the knowledge that an Althusserian kind of theory is still being produced and impressed that Resnick and Wolff produce it about as well as this can be done.

ALAN CARLING

University of Bradford

Meaning and Method in the Social Sciences: A Case for Methodological Pluralism. By Paul A. Roth. Ithaca: Cornell University Press, 1987. 250p. $26.95.

This is a book written by a philosopher for philosophers, but political scientists will benefit from its insight into contemporary philosophy of social science. The intellectual trickling down of methodological wisdom from the broad concerns of philosophers to the empirical research of social scientists has had a significant impact on political science throughout its history. The usual time lag in breakthroughs across fields suggests that just as the implications of logical positivism shaped the political science of the twentieth century, the current philosophical debates over the demise of logical positivism will influence the political science of the future. Roth's work identifies a good starting point for a discussion that could pave the way for progress in our discipline.

Roth proceeds through examination of W. V. O. Quine's critique of logical positivism and the subsequent responses. The details of philosophic interpretation and debate are not of immediate interest to political scientists, but the basic issue is critical. Essentially, the problem of scientific explanation revolves around a disjunction between the data-gathering and explanatory functions of research. The logical implication of empiricism is that knowledge of the world can be obtained only through sensory experience. But how can this neurophysiological epistemology be the basis of the social communication required to present findings as evidence to the scientific community? If we take empiricism seriously, evidence gathering is wholly an individual task, yet communicating it accurately to others is a sine qua non of science. Scientists are confronted with the need to move from personal experience to social expression without relying on question-begging social assumptions. Philosophers have focused on language and learning to explore the implications of this process.

Social scientists have addressed this problem through the acceptance of theories (or paradigms) that establish a set of givens that serve as a foundation for communication. Unfortunately, the positivist view that theory develops in an orderly, evolutionary manner is not justified in view of Quine's critique. All theories are underdetermined, that is, there is insufficient empirical data to rule out all but one possible theory, hence multiple theories are always available to account for observed experiences. This makes belief in the inevitability of continuous scientific progress difficult to sustain, and Thomas Kuhn's rejection of an evolutionary view of science drove the nails into the coffin of positivism for most social scientists. However, Roth argues that social scientists have misunderstood the implications of Kuhn's thesis, and that rather than reexamining basic claims to knowledge, social scientists have attempted to redefine science. By assuming a basic similarity between the natural and social sciences (an assumption not made by Kuhn, Roth points out) social scientists have moved from Kuhn's observation of the paradigmatic qualities of natural science to the conclusion that the establishment of a dominant paradigm is a necessary precondition of a science.

This response to Kuhn has produced one of the two major strains of postpositivist social science discussed by Roth: the unity-of-method thesis. By defining science as the application of one particular method, theoretical confusion is ruled out by insistence on methodological exclusivism. Roth chastises this view as projecting a false image of science, though he charitably downplays the possibility that this is a product of insecurity over the relative success of the natural sciences compared to the social sciences. In any case the intellectual underpinnings of this position are extremely suspect given twentieth-century critiques of positivism and the underdetermination of theory.

The second major strain of contemporary social science is presented as a response to the unity-of-method thesis. Roth describes "the strong programme" approach as a form of the sociology of knowledge, a perspective most prominently associated with critical theory for political scientists. In this view theory is wholly a function of the social context of the scientist; no possibility of getting outside social constraints is recognized. Roth labels this collapsing of data and context "voodoo epistemology," concluding that its primary implication—the impossibility of social science—is unnecessarily pessimistic. While the reductionistic claims of the unity-of-method approach are Roth's primary target, he is unwilling to relegate social science to an amorphous arm of the sociology of knowledge. His analysis portrays social scientists in an epistemological dilemma, caught between a rock and a squishy place.

Roth's solution is the methodological pluralism that has previously been promoted by Paul Feyerabend and others. His purpose in this volume is a negative one, however; rather than outlining this option, he rejects the alternatives. As a result Roth's argument should be used as a point of departure rather than a closed discussion. An embrace of multiple perspectives and methods, with science seen as the sum of a wide range of questions and efforts, may be the logical solution to the complex difficulties that confront the social sciences. For progress to occur along these lines, however, social scientists must become more familiar with serious work like that produced by Roth.

JAMES L. CURTIS

Pennsylvania State University

Ordered Liberty and the Constitutional Framework: The Political Thought of Friedrich A. Hayek. By Barbara M. Rowland. New York: Greenwood, 1987. 148p. $29.95.

Friedrich Hayek has been the most persistent and intrepid of contemporary political philosophers. From the publication of his polemical *The Road to Serfdom* (1944) to the conclusion of his *Law, Legislation, and Liberty* (1979), when Hayek was in his eightieth year, his political theory and the grand traditional format in which he chose to cast it were distinctly out of vogue, though this prolific and unflagging champion of the economic and political outlook of classical liberalism resolutely ignored the vicissitudes of doctrinal fashion. Hayek seemed to many, as Anthony Quinton noted in 1967, like a magnificent dinosaur oblivious to the intellectual laws of natural selection. But that was pre-Rawls, and so pre-Nozick. It was also before the revival of the doctrinal fortunes of a highly energized form of libertarian conservatism in the United States and Great Britain. In the 1980s, Hayek was to find himself heralded as an important intellectual precursor of the new spirit of libertarianism and in practical politics, of Thatcherism.

There is already a large secondary literature on Hayek. The novelty of Barbara M. Rowland's study of his political thought arises from its attempt to place Hayek in the context of the so-called communitarian critique of liberalism associated with Sandel, MacIntyre, Taylor and others, with which the author is in evident sympathy. It is a feature of many commentaries on Hayek's political theory, as of the communitarian-versus-libertarian debate, that the reader is entreated to take sides. But while Rowland aims to deploy communitarian arguments in a close critique of major tenets of Hayekian liberalism and of the libertarian position in general, she tries her best to adopt an ecumenical tone, in line with her professed belief that each side stands to benefit from the insights of the other.

Rowland's book concentrates on the three volumes of *Law, Liberty, and Legislation,* which appeared between 1973 and 1979, for she detects a telling theoretical tension in this trilogy. The highly sympathetic emphasis in the first volume on the role of cultural evolution in abetting the gradual emergence of the rules of abstract order, or *cosmos,* which Hayek argues are requisite for the flourishing of the liberal principles of justice and political economy, points to a profoundly conservative element in his thought. On the other hand, there are in his closing volume contrasting proposals more critical rationalist in character, involving the adumbration of a constitutional "model" intended to provide institutional defenses for the rule of law, or *nomos,* and for the self-generating market order, or *"catallaxy,"* which Hayek believes to have been steadily undermined by distributivist planners and the interventionist state.

Rowland's view regarding Hayek's simultaneous embracing of these positions is that it stems from a too-narrow vision of liberty that is incoherent because it rests on an ambiguous grasp of the nature of the essential self. As a communitarian, she favors a "situated" self in place of the spurious "unencumbered" self of liberal discourse; for, she submits, when Hayek talks of the abstract speheres of *cosmos, nomos,* and *"catallaxy,"* he seems to have in mind atomistic, self-contained individuals who act on the basis of purely rational knowledge; whereas when he turns his attention to the importance of tacit knowledge and the evolution of cultural traditions in the progress of liberal civilization, his discussion implies instead a more fully social view of individuality and one profoundly indebted to the ongoing life of the society in which it is nurtured. Rowland's suggestion is that Hayek and other liberals abandon their narrow conception of tacit knowledge and the thin notion of morality that it yields, in order to theorize a richer view of freedom in which proper emphasis is placed on interactions and relationships that are not strictly exchange- or market-dominated.

Rowland has mounted a thoughtful prosecution of Hayekian liberalism in a short book. But a close consideration of her concluded communitarian brief yields the Scottish verdict of *not proven.* She seems wide of the mark in suggesting that Hayek fits "the category of philosophers who ultimately seek a return to security" (p. 124), a charge one might have thought, on the available evidence, more successfully prosecutable against the communitarians. There is a tendency throughout the book to exaggerate the extent to which, in Hayek's later work, "he minimizes the role of the individual . . . and emphasizes the necessity of submission to tradition in an unreflective way" (p. 82), as there is also to make too much of what is in fact a brief, if perhaps slightly embarrassing, twenty-page excursus ("a sort of intellectual stand-by equipment," as Hayek has called it) relating to his constitutional model.

Most commentators have agreed that Hayek's most representative work of political theory is *The Constitution of Liberty* (1960). And Hayek himself, in a "consolidated preface" to a new single-volume edition of *Law, Legislation, and Liberty* (1982), is emphatic that the late-in-life trilogy is "definitely supplementary to and not a substitute for [*The Constitution of Liberty"]* (p. xix). It was in the well-known postscript of *The Constitution of Liberty,* "Why I Am Not a Conservative," that Hayek, abjuring the use of the terms *liberal* or *libertarian* to describe his political outlook, declared himself to be an unrepentant Old Whig, with the emphasis definitely on the *Old.* Rowland makes no reference to this important self-identification, an omission that is revealing, perhaps, of why her book has an air of reinventing the wheel, for setting aside the crude variety of laissez faire stereotype so commonly deployed in liberal bashing, Hayek's project, like that of the Whigs and their classical and contemporary liberal intellectual descendants, has been, in the course of a long career, a dogged attempt to render an account of political morality and freedom that is at once individual and social—if not quite libertarian and communal—in character.

GEORGE FEAVER

University of British Columbia

Person and Polis: Max Scheler's Personalism As Political Theory. By Stephen Frederick Schneck. Albany: State University of New York Press, 1987. 188p. $44.50 cloth, $14.95 paper.

My favorite Max Scheler story is about the morning on the Piazza in Ascond when he suddenly cried out to a friend, "I have him." "Who?" demanded the surprised companion. "Man," replied the ecstatic philosopher who had hurled defiance at positivist objectification, materialist reduction, and idealist formalization of man. Stephen Schneck turns to Max Scheler's multifaceted and fragmentary account of man as a basis for a comprehensive political theory, deemed superior to the "ego-based" politics of Marxism and liberalism. The author perceives an affinity between Scheler's concept of person and Aristotle's definition of man as *zoon politikon* (p. 2). The essay attempts to bring into focus certain aspects of the contemporary turn toward "praxis." Schenck defines praxis as, "the meeting ground of the way things are and the way things could be" (pp. 1, 134). The first two chapters present an intellectual biography of Max Scheler and a sketch of the Schelerean notion of person as a transcendental, nonobjectifiable "concrete unity of acts" corresponding to an objective order of values. Person, as such, forms a nucleus around which coalesces an intriguing picture of an authentic, praxeologically competent political subject. A table of parallel typologies (p. 55) provides a helpful (if incomplete) illustration of Scheler's philosophical anthropology explaining the origin of person. Thus, in contrast to Kant's formalistic concept of man as "the x of some rational action," Scheler's personalism promises a flesh and blood radical humanism based on the "inestimable dignity and absolute uniqueness of the other as a person."

The third chapter explores novel avenues for understanding political order, equality, and power. The foundational, pretemporal and cosmic order of value-ideals—ranging from pleasant, useful, and vital to noble and sacred—guide the new political vision. The hierarchy of values is reflected not only in human genesis and history but above all in the "heart," the "counter-image of the cosmos of all possible things worthy of love . . . a microcosm of the world of values." Schenck realizes that the idea of a value-imbued personalist politics, that is, a politics without "domination, competition and calculation," may sound odd, even "Quixotic," to the modern ear. However, he attributes this effect to "our embeddedness within the limits of our time" (p. 84).

The final chapter addresses the common assumptions of modern political paradigms: bourgeois liberalism, bourgeois Marxism and "the mass politics of the extreme right and left." These paradigms all serve the "new man," the "bourgeois man" in whose mind "the absolute order of values has been turned on its head" (p. 96). Schneck gauges the deficiencies of bourgeois liberalism and bourgeois Marxism against the standards of personalism. He also underscores their procedural and substantive similarities.

In his concluding remarks Schneck assesses the ambiguities of Scheler's legacy. He regards the recent linguistic, structural, and psychological philosophies as "quasi-transcendental" efforts to verify or oblate the Schelerian person-value duality and the assumption of a self-evident, objective order of values (pp. 139-40). The essay closes with the expression of a Schelerian concern for the realization of "the full reality-existence and promise of person" (p. 140).

Schneck's "person and polis" accomplishes the Herculean task of rising above the chaos of Max Scheler's manifest politics, varying interests, and unfinished projects. The singular contribution of the book, however, lies not in its postindividualist concern with the dignity and solidarity of persons but in its radical expansion of the notion of political theory beyond the ancient, the modern, and the Schelerian imagination. Scheler's political vision, contrary to Schneck's occasionally unconvincing generalizing attempts, was restricted to the lower, that is, useful and vital, values inherent in distributing power and generating well-being. Thus the state would remain merely a negative condition, "the *conditio sine qua non* of a factual world of cultural goods." Max Scheler contends that the ethos by which the state fulfills its tasks stems not from the state itself but from the "spiritual collective persons behind it." Scheler's ideal society, I suggest, constitutes a *Nova Civitas Dei.* It is a society that supplants the Augustinian, symbiotic indifference toward the city of man with a finely calibrated unity in whose embrace "aristocracy in heaven does not preclude democracy on earth." Scheler's politics, then, while minimalist, inherits and fleshes out the suprapolitical values latent in Locke's *Second Treatise on Government,* Madison's *Federalist Papers,* J. S. Mill's *On Liberty,* and Tocqueville's *Democracy in America.*

Let me add in conclusion that Stephen Schneck's explicit politicoethical commitment and meticulous scholarship – not withstanding the unanswered questions and shortcomings that usually accompany projects of this magnitude – resonates with Max Scheler's final words, "We must not wait for theoretical certainties before we commit ourselves. It is the commitment of person himself that opens up the possibility of 'knowing'."

MAHMOUD SADRI

New School for Social Research

The Trial of Socrates. By I. F. Stone. Boston: Little, Brown, 1988. 282p. $18.95.

Intending originally to write a historical account of the freedom of thought, I. F. Stone was held fast by the spectacle of Socrates' trial and execution. To find out how this could have happened in a society as free as Athens, Stone renewed his study of Classical Greek and immersed himself in the ancient texts. The result is described as "the Athenian side of the story" (p. xi). Xenophon and, especially, Plato have left us speeches in Socrates' defense; Stone presents "the missing case for the prosecution" (p. 5).

Stone believes that Socrates despised the Athenian democracy. In opposition to the Greek ideal of equal citizens ruling and being ruled in turn, Socrates believed that "virtue is knowledge" and so looked down on the uneducated common man. Because he believed that government was a craft like other crafts, he thought that it should be entrusted to those with the relevant technical qualifications: "He and his disciples saw the human community as a herd that had to be ruled by a king or kings, as sheep by a shepherd" (p. 39). Stone believes that Socrates helped to turn his associates against the Athenian democracy. When Athens was shaken by a series of "political earthquakes" in the closing years of the fifth century, the Athenians turned on Socrates as an enemy of democracy. Even then, Stone believes, Socrates brought his death upon himself. He refused various means of avoiding his fate, including basing his defense upon the Athenian ideal of freedom of speech, which "might easily" (p. 198) have gotten him off. Accordingly, in chapter 16, Stone gives us his own "Apology of Socrates" – what Socrates should have said.

As a work of popular history and entertainment, *The Trial of Socrates* is unfailingly energetic. It is written in pungent journalistic style and should inspire wide discussion. A large readership should also benefit from the repeated reminder of the gulf that separates Plato's political thought and the ideals of modern liberal democracy. There are some interesting questions for the specialist as well, for instance, the extent to which Athens actually did promote free thought and speech (chaps. 15 and 17 and epilogue), and why Socrates did not resist political evils more vigorously, whether those of the Athenian democracy or, especially, those of the Thirty Tyrants (chap. 8).

Stone is surely to be commended for the great effort that this book represents. But effort alone is not always enough. Because of the widespread disagreements that pervade classical scholarship, he preferred to rely on primary sources (p. x). But his reading of these is skewed by twentieth-century concerns. Stone's idealization of Athens – "the most open city of antiquity, and perhaps of all time" (p. 81) – is largely a caricature, based heavily on Pericles' funeral speech in Thucydides' *History.* Stone has explored the scholarly literature on freedom of speech and related issues, but a perusal of even the standard sources on other aspects of Athenian politics—works by Finley, Ehrenberg, Jones, and so on—would have led to a more rounded picture.

More serious is Stone's refusal to take Socrates seriously as a philosopher. Socrates' philosophic mission was based on an ideal of human perfection: the need to pursue wisdom and to "care for one's soul" (*Apology* 29d-30b, 36c). But of the nature of Socrates' moral ideas Stone's readers receive hardly a clue. Stone has made little effort to present the Socratic doctrine of the soul. The so-called Socratic paradoxes, that "virtue is knowledge" and "all wrongdoing is caused by ignorance" are brushed aside when they are discussed at all (pp. 66-67, 87-89). Socrates' analogy between virtue and crafts—and so the *basis* for his criticism of democracy—is hardly discussed. There is an enormous, rich literature on Socrates and his philosophy that Stone has barely touched.

According to Plato, Socrates embodied the irony that pervaded his speech. Outwardly he was unkempt and squat, with a snub nose, and walked like a marsh goose. But beneath this appearance dwelled the "images of gods" (*Symposium* 215b). Socrates spent his life chaffing and making fun of his fellowmen (216e); his talk was of "pack-asses, smiths, cobblers, and tanners" (221e). But of course Plato believed that he was "the best and wisest and most righteous man" of his time (*Phaedo* 118a).

Stone's Socrates is less complex. He is a clown and a nuisance. His philosophizing is "stratospehric nonsense" (p. 72), "a metaphysical wild goose chase" (p. 75). His argumentation is contentless destructive dialectic, a bag of sophistic tricks (chap 5). The ideas of the *Gorgias* are "poisonous nonsense" (p. 92). In short, "An unmistakable social snobbery lurked behind Socrates' scornful dismissal of Athenian democracy" (p. 117).

It is perhaps to be expected that Socrates will always be viewed quite differently by different people. There is undoubtedly an element of truth in the portrait that Stone presents. But it would take far more than "the golden haze of the Platonic dialogues" (p. 136) to explain how Stone's Socrates came to play a pivotal role in the history of Western philosophy.

GEORGE KLOSKO

University of Virginia

The Recent Work of Jürgen Habermas: Reason, Justice, and Modernity. By Stephen K. White. Cambridge: Cambridge University Press, 1988. 190p. $29.95.

As one of the foremost theorists of our time, Jürgen Habermas has given rise to a great many commentaries. Stephen White's is one of the better ones. Its value lies not only in his reconstruction of some of Habermas' more complex ideas, but also in White's efforts to defend them.

White's greatest service is in communicating to the reader the concepts and arguments of Habermas' most recent and still largely untranslated works. Some of the more important concepts discussed include the "communicative" versions of action, competence, rationality, ethics, and sociation. In each case White contrasts them with older or more widely held conceptions. In the clash their respective strengths and weaknesses are revealed.

Exposition of these and other concepts is warranted, however, for reasons other than their originality. These concepts also serve as the foundation for Habermas' theory and qualified defense of modernity.

According to White, Habermas' approach to the problem of modernity is best characterized by its goal of developing "*standards* which allow modernity to interpret itself in a way which is self-critical, but which gives some basis for *normative self-assurance*" (p. 91, my emphasis).

Such a task requires at least two others. One is "completion" of Weber's concept of rationalization. Rationalization, Habermas argues, must be multidimensional, capable of revealing not only the costs of modernity (loss of meaning, disenchantment) but its benefits too. For Habermas, these benefits amount to enhanced (but, as it turns out, still inhibited) "learning capacities" for mankind. One may very well wonder, however, whether this is indeed a completion of Weber's project. As White correctly points out, "Specialists will find much to contest" (p. 93).

The second task is construction of an altogether different and more accurate theory for illuminating and explaining the " 'pathologies' of a rationalist society" (p. 91). The distinction between base and superstructure will no longer suffice, nor will the approaches commonly associated with strict functionalism or systems theory. Study of the traditional "steering-capacities" (money and power) of advanced capitalist societies must be balanced by even closer attention to largely symbolic processes of reproduction. Habermas' concept of the "life-world" is his means for uncovering the needs, "action orientations," and structural requirements for this dimension of social life.

This two-level approach allows Habermas to claim, unlike Nietzsche or Weber, that modernity is not fundamentally contradictory. It is instead distorted. Through one-sided development, White shows us, modernity has taken on its self-limiting, instrumentalist characteristics. Habermas' concept of "colonization of the lifeworld" is his vehicle for revealing just how this distortion has taken place and how it might be checked.

White's study, however, is more than exposition. It is also a defense. White correctly recognizes that to defend Habermas is to defend his theory of a universal pragmatics; that is to say, the assertion that through analysis of the inevitable assumptions of speech, universal moral principles may be both discerned and justified. Added to this is White's conviction (with Habermas) that justification of universal moral principles can be accomplished in a "nonfoundationalist" way.

White's arguments are far too subtle to be treated fairly here. Nevertheless, two features of his defense stand out. One is the argument that Habermas' universal pragmatics is correct not just because Habermas has accurately defined the unavoidable presuppositions of speech but because universal pragmatics more fully accommodates that which White deems "*constitutive* of what we understand as *human* action" (p. 14). As a result, "justice" can be viewed once again in universal terms, founded, so to speak, "in the nature of things" (p. 22). It seems to me, however, that this defense is based on something yet to be demonstrated.

The other outstanding feature is White's claim that Habermas avoids the pitfalls of traditional Kantian foundationalism. How so? For White, the answer lies largely in the limits Habermas has imposed upon himself. Unlike Kant, Habermas makes no effort to elucidate the conditions of all possible knowledge. Instead, Habermas restricts himself only to those conditions "for the validity of meaningful expressions and performances," that is to say, to "rational reconstructions of the know-how of subjects, who are entrusted to produce valid expressions and who trust themselves to distinguish intuitively between valid and invalid expressions" (p. 129). Moreover, neither White nor Habermas regards these "reconstructions" as a priori. They are instead hypothetical, subject to empirical confirmation (p. 130).

The success of these and other claims, I would argue, is still a matter for debate. However, White does raise the debate to another level. Insofar as this is the case, critics and proponents alike would do well to study his efforts.

THOMAS CLAY ARNOLD

University of Texas at Arlington

AMERICAN POLITICS

Politics by Humans. By James David Barber. Durham, NC: Duke University Press, 1988. 517p. $59.50 cloth, $19.95 paper.

The Mask of Command. By John Keegan. New York: Viking, 1988. $18.95.

Leadership: Multidisciplinary Perspectives. By Barbara Kellerman. Englewood Cliffs, NJ: Prentice-Hall, 1984. 288p. $19.95.

Political Leadership: A Sourcebook. By Barbara Kellerman. Pittsburgh: University of Pittsburgh Press, 1986. 462p. $34.95 cloth, $14.95 paper.

The hollow appeals to "leadership" made during the recent presidential campaign tempt one to conclude that leadership is like a good joke, wonderful to encounter but useless to talk about. Political science must resist that temptation. Such paeans to leadership are really an indication of how difficult its practice has become and therefore how important it is to gain a better grasp of its fundamentals.

The full contours of the debate about leadership are well delineated in the two collections edited by Barbara Kellerman. Certain topics that have preoccupied scholars, like the "great man" theory of history and the effort to define a leader's "mental health" now appear shopworn. But the volumes succeed in focusing the reader's attention on the two key problems posed by the study of leadership: the character of leaders and the principles which define good leadership. Those two themes are pursued more fully in recent works by James David Barber and John Keegan.

Politics by Humans is a compilation of Barber's periodical writings on leadership along with a few selections from his books. Although he covers a variety of topics, including the media and the behavior of state legislators, his central concern remains the effort he first staked out in *The Presidential Character* (1972) to predict presidential performance by analyzing the personalities of presidents. In *The Mask of Command,* Keegan focuses on the imperatives of successful military leadership and assesses the performance of four great commanders—Alexander, Wellington, Grant, and Hitler—on that basis.

Barber's well-known analysis centers on the interplay of what he calls character and style. He classifies presidential character on the basis of two aspects of personality: level of activity and type of affect. This twofold schema yields four different character types (active positives, active negatives, passive positives, passive negatives).

Barber defines style as the manner in which the president allocates time, attention, and affect among the three central presidential tasks: rhetoric, decision management, and personal relations. Style is not a simple derivative of character. It is also linked to the particular and to some degree accidental circumstances that mark the individual's earliest political success. This normally occurs in late adolescence or early adulthood. Having found a way to succeed, the individual falls back upon this same method to cope with later crises. Because the key elements of character and style are discernable early in a politician's career, they are predictive of how an individual will behave as president. Therefore, they can fruitfully be used to evaluate rival presidential candidates.

Barber's approach has several virtues. He pays careful scholarly attention to obscure presidents like Andrew Johnson and Calvin Coolidge, who have as much to teach about the relationship between character and the presidency as do their more luminous counterparts. Recalling the political lessons learned during early adulthood can help to explain a president's seemingly anomalous behavior in times of crisis. Character is in fact the single most important quality to be judged about a presidential candidate, far more important than the stands he takes upon the oft-ephemeral isues that dominate campaigns. In this era of mass politics, the mass media must take the lead in delving into the character of presidential aspirants, and Barber is right to make political science responsible for instructing journalists in this art.

This approach suffers from its psychological reductionism. It misses essential differences between presidents because it ignores their politics. For example, Barber never mentions the obvious fact that all Democratic presidents in the twentieth century have been "active" while all the "passive" presidents have been Republicans. He classifies both Nixon and Johnson as "active negatives," claiming that those qualities of character account for their stubborn adherence to destructive policies. It may well be true that these two leaders shared important psychological affinities, but these should not serve to blind us to the crucial differences in their public behavior. Barber is free to disapprove of Johnson's Vietnam policy, but he has no grounds for treating it as a psychological aberration. The most remarkable attribute of that policy was the clear-cut set of limits that Johnson imposed upon U.S. involvement and his resolute opposition to calls for total mobilization and "all-out" war. Whereas Nixon had to be hounded from office under the threat of impeachment, Johnson resigned voluntarily despite enjoying a reasonable prospect of reelection.

Labeling both Eisenhower and Reagan as "passive positives" is equally misleading. Ike's extraordinary power to mobilize activity is evident in the magnificent generalship he displayed in World War II. He was not a *passive* president; he was a conservative. He did little on the domestic front because he believed that little ought to be done. By contrast, Reagan has presided over the most programmatically ambitious Republican regime in the twentieth century, far more activist than that of the only Republican active positive, Theodore Roosevelt. Unlike Eisenhower, Reagan is not a conservative but a radical reformer of a right-wing cast. Their politics, not their personalities, account for the crucial differences in how they governed.

Keegan's entire analysis is predicated upon the essentially political nature of leadership, the essence of which he calls "the mask": "The leader of men in warfare can show himself to his followers only through a mask, a mask he must make for himself, but a mask made in such form as will mark him to men of his time and place as the leader they want and need" (p. 11).

He depicts the masks worn by each of the four commanders and shows how each provided a particular response to the imperatives of command. Alexander was the hero, leading by example, charging headlong into battle, inspiring his men to charge forward by their desperate longing to save his life. Wellington was the antihero, shunning the theatrics of heroism in favor of the cool detachment appropriate to a gentleman. Grant wore the mask of the republican, commanding through consent. Hitler donned the macabre garb of the false hero, hiding behind the lines while his propaganda machine trumpeted his valor and solidarity with the men.

Mask is a way of talking about character in nonreductionist terms. While it is unlikely that a successful mask could be fashioned that was at odds with the personality of its wearer, it is not simply an attribute of his or her personality. It also incorporates a political judgment about one's followers and about the strategic problems one faces.

All of these commanders won battles, but only Wellington and Grant ultimately survived and were victorious. *Their* masks enabled them to respond to command's imperatives more successfully. The limits of time and energy mean that no one commands alone. A general depends on his close associates to advise him but more importantly to serve as a window to his troops. They broadcast his views to the men and convey the attitude of the troops back to him. Republicans and gentlemen can assemble such a team. Heroes cannot. Alexander's intimates were also her peers. They too were heroic individualists, courageous in battle but constitutionally unsuited to subordinating their will to his. As befits a false hero, Hitler cultivated the sycophancy of false friends.

The most difficult command imperative is that of example. As his wounds attested, Alexander was always at the front, inspiring his men by the risks he incurred. But by being first into the fray, he lost the ability to command as soon as the battle had begun.

Hitler avoided the battlefield entirely, to avoid personal harm but also to be able to make decisions in an atmosphere free of the distractions the battlefield imposes. He relied on the radio and telephone to keep him informed. Despite his own experience in the trenches of World War I, he chose to emulate that war's "chateau generals," who fought the war at the farthest remove and sought to make of it a strictly technocratic enterprise. Because he could not *see* the battle, he was impervious to its critical intangibles—Were his officers providing a good example to the men? How much fight did his troops and the enemy's have left in them?

Wellington and Grant shunned the hero's bravura, but they still found means to maintain a tactile grip on the field of battle. Unlike Hitler, they did not confuse knowing with seeing. Although the advent of rifle and cannon prevented them from being at the front, they stayed sufficiently close to observe it directly and to provide concrete proof of their physical courage.

The import of Keegan's work is by no means limited to military matters. Campaign consultants, pollsters, and management specialists are as prone as "chateau generals" to substitute data for observation and expertise for example. He returns the study of leadership to its fundamentals: the creation of a shared identity based on common experience, the formation of a team of aides that shares the leader's aspirations and does not fear to tell the truth to power, and the ability to penetrate abstractions and retain the "touch and feel" of the political environment.

Most importantly, the idea of "mask" provides an excellent principle on which to ground future leadership studies. It is sufficiently broad to incorporate psychological and biographical insights, but it remains clearly focused upon the essentially relational nature of leadership. Each mask must be fitted to the particular sort of followers the leader seeks to engage. This idea can help political scientists resist the lures of reductionism by reminding us that leadership is a public practice and that the study of it must rest on public principles. Our discipline is indebted to this great military historian for providing such a timely political lesson.

MARC LANDY

Boston College

Showdown at Gucci Gulch: Lawmakers, Lobbyists, and the Unlikely Triumph of Tax Reform. By Jeffrey H. Birnbaum and Alan S. Murray. New York: Random House, 1987. 291p. $18.95 cloth, $9.95 paper.

In what I expect will become the most widely adopted legislative history since *The Dance of Legislation*, these two *Wall Street Journal* reporters tell a good story well. From its idealistic but unpromising beginnings in the Bradley-Gephardt tax reform bill of 1982, the most radical reform of the nation's tax code in contemporary times, what was to become the Tax Reform Act of 1986 survives public indifference; intense opposition from ostensibly powerful and surely well-heeled special interest groups and their pin-striped, Gucci-clad Washington lobbyists; calculations of partisan advantage emanating from the Reagan White House and Democratically dominated Congress; the clash of image-conscious, hungry egos; and the potential for policy gridlock that we have learned to be typical of the modern day Congress. And although a full assessment of the resulting social and economic effects has yet to be made, Congress, with this law, does appear to have removed more than four million poor persons from the federal tax rolls, demonstrated its institutional policy-making capacity, and shored up the legitimacy of the political system.

The story is surprising, given what may be only the folklore that most of us have come to accept as scientifically substantiated truth: the contemporary Congress—unable to affect comprehensive or coherent policy because of its high degree of institutional fragmentation, tied into a terminal case of policy gridlock by the demands of special interest groups, and not sufficiently integrated by party organizations or any other mechanism—lacks the capacity to solve serious social or economic problems through the making of public policy that is either nonincremental or distributional.

In their mostly chronological and anecdotal manner the authors tell the tale that couldn't be. Contrary to what modern textbooks, such as Ripley's *Congress: Process and Policy*, lead us to expect, Congress in this case appears to have performed its function of lawmaking better and more responsibly, at the expense of its normally dominant function of representation. Indeed, this success in lawmaking came about because, in part, members of Congress ignored many of the demands of narrow special interest groups. Contrary to the complaints of self-appointed spokespersons for what they view (from either the left or the right wing) as the underrepresented diffuse interests of broad constituencies, Congress, in this case, appears to have been quite attentive to what members took to be the general public's underlying or latent concern with the fairness of the tax code and the legitimacy of the government it funded, even though members, on their trips back home, couldn't find many constituents who acted as though they cared very deeply about the issue.

Moreover, the authors tell the story in a way that is closely detailed but without the sort of irritating repetition that often mars works of this sort. In the telling, we see both the hedgehog and the fox: the operation of institutional and contextual forces (Congress's felt need to reassert itself and public antipathy toward taxes and government in general) and the impact of happenstance (the occasion for a few, but not too many, beers to lubricate the legislative process).

Since Birnbaum and Murray accomplish so much so well in this excellent case study, it may appear mean-spirited to point out their shortcomings, especially since they were probably editorially imposed. Also, it's a little unfair to criticize them for not accomplishing what they did not set out to accomplish. Nevertheless . . .

The result is seriously underconceptualized, although, understandably, this was not the book's primary concern. It does not make any systematic attempt to explain what happened, either by reference to broader theoretical frameworks or in comparison with other legislative histories.

Ironically, however, *Showdown's* shortcomings are a part of its value: by telling well a substantively interesting story without forcing it through their perceptual screen, the authors provide grist for our conceptual mills. The task of teasing out and testing hypotheses, both within this case and in others, remains the students'.

Some such conceptual concerns are straightforward: How does one account for this legislative result? Was tax reform a unique and therefore isolated case of effective policy-making? What, if anything, does this case suggest or tend to confirm about the legislative capacity of the contemporary Congress? Is it more generally true than we have recently supposed that institutional reform is not a prerequisite for fundamental change in policy?

Finally, *Showdown* suggests that those who hold that Congress is dominated by special interest groups have overstated the case. In this instance at least, it was the constellation of special interest groups, and not Congress, that suffered gridlock. By each pursuing their own narrow interest, lobbyists were blind to opportunities to unite in the sort of opposition that could have defeated tax reform. Because their focus was narrow, their influence was slight; because there were so many competing interest groups, no one group had disproportionate influence. Maybe editorial writers and op-ed columnists should reread *Federalist* 10: maybe it is true that the answer to the problems of democracy is more, not less, democracy.

PAUL A. DAWSON

Oberlin College

Water and Poverty in the Southwest. F. Lee Brown and Helen M. Ingram. Tucson: The University of Arizona Press, 1987. 226p. $19.95 cloth, $12.95 paper.

Command of the Waters: Iron Triangles, Federal Water Development, and Indian Water. By Daniel McCool. Berkeley: University of California Press, 1988. 321p. $38.00.

These two books address the relationship of water policy and minority groups in the United States, in particular, American Indians anywhere and Hispanics in the Upper Rio Grande Valley and the Tohono O'odham nation (formerly the Papago tribe) in Southern Arizona. For Brown and Ingram, the question is whether water policy can resolve poverty for Hispanics and Indians in the Southwest; for McCool the question is whether Indians can make effective their reserved water rights and thereby improve their economic condition.

The authors of both books take up interesting but different theoretical questions of interest to political scientists and policy analysts: for Brown and Ingram, it is how water is evaluated—as a commodity in the marketplace or as a community resource; for McCool, it is whether the iron triangle of water resource development has as much validity for Indian water interests as it does for non-Indian interests and whether the iron triangle concept retains its validity in the face of serious challenges that have arisen during the past decade.

Both are superbly researched but their research has taken them in distinctly different directions. Brown and Ingram have undertaken two case studies of minorities in the Southwest and have particularly investigated minority conceptions of the value of water and what it means to them and their culture. In the cases of both the Hispanics and the Tohono O'odham, they find a firm linkage between the availability of water and the protection of their cultures. Water is not just a resource or a commodity in the marketplace; it is fundamental to their way of life, to the protection of the values implicit in their self-identification. Thus the authors emphasize the necessity of minority participation in water policy decision making because they alone can define the relationship of water to their values. Failure of water policy to deal with their poverty in the past is found to be directly related to failure to provide opportunities for participation.

McCool takes a more comprehensive approach, evaluating the prototypical iron triangle of water policy making as it has developed for both the Indians and the non-Indians. He traces the development of both triangles, reaching the conclusion that despite internal dissension within the non-Indian triangle and attacks from without, it has been and may yet remain a powerful engine of policy-making. With respect to the Indian triangle, he reaches the not-surprising conclusion that it has never been strong and has in recent years become essentially a bureau-client relationship of little influence in federal decision making. In fact, it seems almost inappropriate to describe it as an iron triangle, given the record of inability to protect its boundaries or forge effective alliances with respect to water.

The answers to the central question the authors pose regarding economic and political equity is complex and to some extent contradictory. McCool argues that the Indian triangle has been weak against the non-Indian triangle for many reasons: Indians are, of course, marginalized from U.S. society generally; seldom have the technical or financial resources to fight effective battles; are not well represented administratively through the Bureau of Indian Affairs or the Department of Justice; lack the statutory basis for their claims for water, having to rely on court judgments that do not receive political support; and are faced with a state water law system that is systematically supported by Congress whenever it acts on water issues.

With such an array of institutional and societal forces opposed to the achievement of their goals, it is understandable that the Indian record of success has been so dismal. Beginning with the famous *Winters* decision, courts have decreed water to the Indians, but Congress has consistently failed to provide them with the resources to claim the water assigned to them. State governments, of course, are inalterably opposed to Indian water rights. McCool paints a dismal picture, and his judgment about prospects for the future of Indian claims is little brighter.

Starting from a different conceptual starting place, Brown and Ingram reach a somewhat more optimistic conclusion. Hispanics in the Upper Rio Grande are somewhat more favorably situated with respect to water rights: they have them and they are reasonably well protected under state water law. The threat to their water rights comes from non-Indian water development interests who want to include them in their projects. Hispanics have effectively organized to prevent the seductive overtures by these interests and their allies in the federal bureaucracy. (One paradox in this picture is the current battle between the Pueblo tribe and the Hispanics over water rights.) The real question for the Hispanics is whether they can organize to create a viable agriculture economy and thus protect their water rights and improve their economic position or whether they will continue their present practice of marginal, part-time farming while holding full-time nonagricultural jobs. Hispanics can apparently forestall big projects, but can they protect their culture against the incentives to sell their individual water rights to the highest bidder?

The Tohono O'odham, too, have water rights, but unfortunately they have been undermined by overmining by municipal and mining interests in the Tucson area. For once, Congress has acted to guarantee Indian access to water but did so only under circumstances in which non-Indian interests had a direct stake in the passage of the legislation, that is, the extension of the Central Arizona Project to the area. The legislation assigns water to the Indians and provides financial resources by which the tribe can develop the water, presumably for agricultural purposes.

Given the sorry record of the federal and state governments in providing the water necessary to make their reservation land productive, this legislative enactment may be the only viable solution available to the Indians. But as McCool points out, that strategy has been tried before, the Indians being included in the non-Indian water triangle because of some advantage to the non-Indians and then finding their projects languishing for a variety of reasons. Brown and Ingram are cautiously optimistic about the likelihood of the success of the Tohono O'odham, but one cannot help harboring doubts at this stage of the game.

The ultimate question asked by the authors, especially Brown and Ingram, is whether some federal water policy will achieve, if not prosperity, some diminution of the poverty in which the Indians especially, but also the Hispanics, dwell. McCool argues that it must provide justice, by which he means recognition of Indian water rights and inferentially improved economic status. Brown and Ingram argue that it can and find evidence for this in the recent Tohono O'odham experience.

Accepting that water has a meaning of community for Indians and the Hispanics under examination here, it seems to me that it has meaning primarily in terms of the traditional societies of which they are a part. Water provides a relationship to land, to customs associated with land use, to myths that are cherished, and to social structures that are revered as part of their heritage. But water may mean something entirely different in terms of poverty, which, after all, is relative. Protection for the Hispanic water right may be desirable, first, if the individual holder wants it. But that protection may simply mean a continuation of traditional practices that have guaranteed a way of life that required off-farm employment and marginal agriculture. Protection and enhancement of the Tohono O'odham water rights may provide water for traditional agriculture, but will it provide the technology necessary to survive in a market economy and thus to eliminate poverty? Poverty is not necessarily related to water, and to rely on water policy to eliminate poverty may be burdening it with responsibilities it cannot carry.

The alternative, of course, is the welfare state, at least for the Indians. One of the paradoxes of the failure of U.S. government to establish an effective water policy for the Indians has been the willingness of Congress to provide them welfare instead. Social services are necessary when unemployment on the reservations is so high, but how much better it would be to supply the Indians with resources, in this case water, and let them pursue their own goals, whether it cures poverty or not!

These books are exceptionally insightful, remarkably well written, and full of both excellent conceptualization and rich detail. The authors know their history, are intimately acquainted with the actors, and are familiar with the economic and political forces that provide the environment of their research efforts. They not only are a pleasure to read but stimulate thinking about what policy really means in its manifold ramifications. For those interested in public policy, these are truly excellent additions to the literature.

DEAN E. MANN

University of California, Santa Barbara

Bureaucratic Discretion: Law and Policy in Federal Regulatory Agencies. By Gary C. Bryner. New York: Pergamon, 1987. 250p. $32.50 cloth, $14.50 paper.

The greenhouse effect, acid rain, rising cancer rates and increased miscarriages around petrochemical plants represent but a few of the many examples of environmental hazards that currently plague U.S. citizens long accustomed to the trade-offs of industrialization. They also reveal the consequences of rapid social deregulation: what happens when "public goods" fight a losing battle against economic growth.

Social deregulation has occurred virtually unnoticed in the last decade, engineered by the executive branch in the most dramatic exercise of presidential power since the New Deal. The election of Ronald Reagan, who campaigned on a platform of "getting the government off the backs of the people," signaled a new era of decelerated power for many federal agencies. In the process, the public got more than it bargained for: less protection, more decisions based on politics than technical expertise, and a general devaluation of government. Reagan and his allies were extraordinarily successful in fulfilling their goals and unraveling the administrative process: their innovative strategy of elevating the Office of Management and Budget (OMB) to the status of regulatory gatekeeper would have been the envy of other recent presidents, who tried, with less success, to control the "headless fourth branch of government" and the executive branch agencies.

The Reagan presidency capitalized on the public's reaction to regulation, which crested in the late 1970s. This was linked to a view of administrative power that viewed government agencies as unrepresentative, arbitrary, and too powerful for the public good. A new book, *Bureaucratic Discretion*, interprets this phenomenon in the context of the administrative process and represents a real contribution to the field. The book traces the components of regulatory power through the separate branches of government, with an emphasis on the statutory base of regulation and the character of its implementation. It is a very useful study, carefully researched and analyzed with skill and depth.

The political science perspective makes this study especially distinctive. The literature on government regulation has been dominated largely by economists and lawyers; most political scientists who have recently tackled this subject have been heavily influenced by economists, and their work reflects their very unique—and virtually uniform—perspective. Bryner does not fall into this trap: instead, he examines in depth the political aspects of regulation—and deregulation—and the results add an important dimension to the field. Bryner discusses, for example, how lawyers and lobbyists prolong regulatory proceedings and exacerbate the already adversarial nature of the regulatory process. He also explores in some detail the most underresearched but most significant area of regulatory politics: the role of OMB and the increasing hegemony of presidential power.

Bryner challenges the conventional wisdom of OMB's regulatory "reviews," by showing the flaws in cost-benefit analysis, which constitutes the methodological basis for OMB's new role in the regulatory process. His critique of the application of what he calls "pseudo-economic" analysis to regulatory decisions shows that this methodology is not analytically neutral when political actors run the numbers. The evidence often reveals this method's biases, such as the claim that reducing the crash-worthiness of auto bumpers saved consumers $42 billion. The fallacy lies in the increased costs on the other side of the ledger: increased insurance rates and repair bills. The federal courts have agreed with this assessment, judging from the reversals of agency actions stemming from regulatory "review."

Bureaucratic Discretion also includes four useful case studies of the most politically controversial agencies: Food and Drug Administration (FDA), Occupational Safety and Health Administration (OSHA), Environmental Protection Agency (EPA), and Consumer Product Safety Commission (CPSC). The most interesting case study deals with the EPA, perhaps the most influential of the agencies discussed. "Congress, White House advisers, and federal courts have all largely developed their views concerning administrative procedures with the EPA clearly in mind," writes Bryner.

The case studies are generally quite solid in analyzing the details of rule making and sorting out the complex roles of the three branches of government. Occasionally, the studies become bogged down in detail, losing sight of the chapter's central thesis, as well as a sense of why these agencies are important in the first place. Also lacking at times is a sense of context. One of the case studies involves an agency that is practically defunct: the CPSC. Why has an agency that regulates consumer products been deregulated to death while the Securities and Exchange Commission has been strengthened? The truth lies somewhere in the political process; an agency's mandate, mission, and ability to survive depends on public acceptance and protection. Evidently, in the current value system, U.S. citizens feel they can protect themselves against unsafe products, but need big government to guard their stock portfolios.

The book's real strength rests on its ability to isolate the problems of regulatory power by tracing that power back to its statutory base and often Byzantine implementation. The author's recommendations are sound and rest on a solid empirical base. He suggests that laws be written to incorporate costs, benefits, and risk assessments; rule making be streamlined; and congressional oversight be more systematic. He also recommends that the nation upgrade its capability to assess and respond to threats to its environment and public health. Most important, the book concludes that future presidents resist the "urge to reshape statutes to their ideological like through informal, managerial means," which only leads to "discouraging compliance with laws and . . . rejecting the democratic formalities on which the constitution and the separation of powers rest."

SUSAN J. TOLCHIN

George Washington University

Nuclear Imperatives and Public Trust: Dealing with Radioactive Waste. By Luther J. Carter. Washington: Resources for the Future, 1987. 473p. $30.00.

Congress and the Nuclear Freeze: An Inside Look at the Politics of a Mass Movement. By Douglas C. Waller. Amherst: University of Massachusetts Press, 1987. 346p. $25.00 cloth, $12.95 paper.

How effective is the U.S. Congress in dealing with the myriad of problems created by nuclear energy and weaponry? Put somewhat differently, to what extent can an institution established in the late eighteenth century cope with the stresses and strains of modern technology? Most students of Congress would applaud the ability of our national legislature to confront change; others would be highly critical. A century ago, Woodrow Wilson set forth one standard for evaluating national legislatures. "It is the proper duty of a representative body," he argued, "to look diligently into every affair of government and to talk much about what it sees. It is meant to be the eyes and the voice, and to embody the wisdom and will of its constituents." Two books by journalists with a keen appreciation for and insight into the legislative process provide us with a partial evaluation of how well the Congresses of the early 1980s have discharged these functions. Not incidentally, both men were initially exposed to Congress as American Political Science Association Congressional Fellows.

Among the major problems created by the nuclear age, four are especially critical: (1) the proliferation of nuclear weapons and the holocaust that would result from their use, (2) the threat or use of nuclear explosives by terrorists, blackmailers, or other criminals, (3) accidents or sabotage at nuclear reactors, such as occurred at Three Mile Island and Chernobyl, and (4) the long-term management of radioactive wastes. (Carter, p. 10). Douglas Waller focuses primarily on congressional response to the first problem. Luther Carter focuses on the fourth, but with inevitable attention to the third.

Carter's book is the more comprehensive of the two. Six years in the making, it tells the story of the nuclear industry and the federal government's flawed and lagging program for disposal of radioactive waste. Projections made by the Department of Energy in 1985, assuming no further growth in nuclear capacity beyond the 115.3 gigawatts (one gigawatt = one billion watts), predict about 41,500 metric tons of spent fuel by the year 2000. Currently, almost all spent fuel remains at the reactor sites with pools of water providing both cooling and radiation shielding. These temporary waste storage pools at the reactors were never intended for long-term storage. Any catastrophic incident like an earthquake could result in the release of dangerous radioactive substances to ground or surface water. Since the 1960s but directly mandated by the passage of the Nuclear Waste Policy Act of 1982, the search is on for deeply mined geologic repositories as the final resolution of the problem.

For nearly six years the search for depositories has generated great political controversy, especially in Nevada, Washington, and Texas. As Carter documents in carefully crafted case studies, NIMBY (not-in-my-backyard) is an almost inevitable concomitant of the problem. Increasingly, attention has focused on Yucca Mountain in Nevada. This proposed site straddles the southwestern boundary of the Nevada Test Site, used for nearly four decades as a testing ground for nuclear weapons.

A concerned citizenry can take little consolation from the experiences of other countries. As Carter develops at length, Europeans—the United Kingdom, Germany, Sweden, and France—and the Japanese have also conducted nuclear projects on an extensive level without adequate means for disposing of the cumulating spent fuel.

In contrast to Carter's external, comparative, and historical analysis, Waller's book focuses on an attempt to pass a nuclear freeze resolution from the viewpoint of a participant-observer. Joining the staff of Representative Edward J. Markey of Massachusetts in 1981, he describes the legislative struggle to mobilize support for H.J. Res. 13, the nuclear freeze resolution in the 98th Congress (1983–84). President Reagan's slowness to move toward arms control provides the initial impetus. Randall Forsberg, an MIT doctoral candidate and peace activist, is credited with the idea for a *mutual* and *verifiable* nuclear weapons freeze. Her proposal, first drafted in 1979, quickly caught the attention of other peace advocates, including the American Friends Service Committee, Clergy and Laity Concerned, and Physicians for Social Responsibility. By 1980 an Ad Hoc Task Force for Nuclear Freeze had been formed and spread rapidly across the nation.

Massachusetts' constituents formed the most active core of the movement. Thus, following Wilson's admonitions, it was hardly surprising that Representative Markey and Senator Ted Kennedy would assume major roles in trying to get nuclear freeze resolutions through Congress. Kennedy, whose campaign for the presidency was once again in high gear by the end of 1981, was joined as a Senate sponsor by Oregon's Mark Hatfield, deeply religious and one of the most dovish Republicans in the Congress. Markey was a four-term Democratic congressman from the working-class suburbs north of Boston, who had already made a name for himself in his opposition to nuclear proliferation.

In the classic genre of "how a bill becomes a law," Waller traces Markey's role in successfully bringing the nuclear freeze resolution over a series of legislative hurdles, including the House Committee on Foreign Affairs. Finally, after days of heated debate, a much-revised resolution passed the House on 4 May

1983 by a vote of 278 to 149. The Senate version, despite the best efforts of Kennedy and Hatfield, was never able to surpass 40 votes in the Republican-controlled 97th and 98th Congresses (1981–84). Waller claims with some justification that the movement could successfully boast a number of accomplishments, even some influence on President Ronald Reagan. To a degree, this debate eventually paved the way for the ratification of the INF treaty in 1988. But by late 1984, for all practical purposes the nuclear freeze movement had all but disappeared from the national scene.

Both books are valuable contributions to a growing literature on government and nuclear power policies. Carter shows how the Nuclear Waste Policy Act proved to be less than a solution and may even have resulted in a political cul-de-sac unless the Nevada site proves out. Waller provides a fascinating account of a movement's growth, initial success, and long-range nadir. Overall, as these two books demonstrate, the role of Congress in the treatment of nuclear policy has been fragmented and episodic but, in the main, responsive to constituency pressures.

ROBERT L. PEABODY

Johns Hopkins University

City and Regime in the American Republic. By Stephen L. Elkin. Chicago: Chicago University Press, 1987. 220p. $35.00 cloth, $11.95 paper.

Elkin's main contribution to the debate about the state in capitalist society has been to combine classical liberalism (e.g., property rights as well as civil rights and civil liberties) with democratic government's concern for widespread political participation and popular control. This Lindblomian combination of markets and politics yields an uneasy mixture that he calls a liberal democratic state. In this volume he focuses on the problems and possibilities of this regime in the United States. Central to Elkin's argument are two main themes: (1) that political institutions help form us as citizens and (2) that the kind of popular participation necessary to guide elected officials in governing a just and prosperous republic comes from the political conflicts that arise in large cities.

Constitutional arrangements and how officials act (whether in the White House or city hall) create institutional opportunities for people to interact with one another and discuss their claims for justice. This "constitutive view" (p. 108) of political institutions is crucial to the idea of a commercial republic. The regime the founders intended is presently in danger in the United States because business interests have been too successful in getting public officials to respond to their short-term economic concerns. In this volume Elkin's best work comes in connection with his discussion of national government (e.g., the decline of Congress as a representative institution, the symbolic nature of the contemporary electoral system, and the rise to prominence of executive and judicial officials who manage governmental affairs outside of the traditional arenas of public debate and popular control).

The problem comes with Elkin's second theme, that the best way to counter the trends leading to a corrupted liberalism is a restructured city government. Some of the reasons that city government is central to his argument—the usefulness of neighborhood-sized units and face-to-face contacts for working out conflicts among diverse populations and the importance of policy domains such as education, law enforcement, and land use to people's everyday lives, and so on—are familiar to students of urban politics. So too are the difficulties awaiting urban reformers determined to empower an urban citizenry. Do mayors preside over local urban growth coalitions or implement policies made by national government agencies and officials? U.S. cities have extremely narrow authority to act, so Elkin must convince the reader that city political systems can solve the problems that citizens bring to government.

His case study of business-government relations in Dallas illustrates the strengths and weaknesses of his approach. The city manager form of government worked as long as business leaders needed only minimal (help from) government. Now that "rearranging land use" (p. 71) has become an important policy domain there, development interests and neighborhood groups both need elected officials who can pull the levers of government. Governments like that in Dallas illustrate Elkin's point that institutions help form citizens' expectations and behavior. But city manager cities seem exceptionally poor soil for Elkin's reforms. In dismantling political parties, business-led reform coalitions helped sever traditional mechanisms of popular control, depoliticized and deemphasized government, and accustomed people to think of themselves as claimants on bureaucracy rather than citizens discussing issues of common concern.

His call for more power to neighborhood-based deliberative assemblies comes after a series of disappointments for those (myself included) whom Elkin would call "friends of the regime." Among the failed attempts to reform city government in the 1970s were challenges to street-level bureaucrats, pressure for participation in the management of federal and local programs by neighborhood organizations, and experiments with community development corporations. What effects the continued rise of minority and women mayors in major urban areas, the slow demise of at-large election systems, and increased concern by middle-class communities with city government services will have on popular control is not yet clear. Elkin doesn't show how to get to the new political institutions he favors or what they will look like. But he does set out challenges to scholars in several subfields of political science.

Those who write about federalism and intergovernmental relations need to consider the consequences of cities acting as agents of the federal government. Others will have to deal with his charge that Congress is becoming less deliberative and its members more like executive branch technocrats. Those concerned with business-government relations and interest group theory may find some of the linkages here that they need to bridge the gap between the pluralism-elitism debate and political science's newer political economy focus. But the biggest challenge for Elkin and his readers will be to follow up on his insights into the political economy of cities. If, as Elkin believes, cities differ significantly from other forms of local government (and the private sector organizations that also deliver public services), he and other political scientists need to advise citizens about how much emphasis to put on increased popular control of government at that level and how much to reserve for national government and politics. I look forward to the contribution that Elkin will play in this ongoing debate.

ARDITH MANEY

Iowa State University

Strangers or Friends: Principles for a New Alien Admission Policy. By Mark Gibney. New York: Greenwood, 1986. 169p. $29.95.

In the United States and other industrialized countries, efforts to devise realistic and fair immigration admission policies require wrenching choices among a multitude of competing demands. The difficulties are compounded by sharply differing public views as to how much "space" should be made available to would-be entrants. Historically, immigrants have been favored depending on their family ties to the United States or the skills they bring. Or, alternatively, groups of people have been admitted as refugees because they have been judged to be victims of political circumstances in their countries of origin. Refugee admission, in turn, has been facilitated by ideological and other affinities. What kinds of responsibilities do the citizens of a prosperous, peaceful nation have toward citizens from repressive, war-torn, and impoverished countries? This work, joining political theory and philosophy, seeks to answer that question.

Obviously, the problem of determining which of the immigrants seeking admission should be granted it raises complicated ethical questions. As the title suggests, Gibney sets himself the task of devising formulas for distinguishing between "strangers" and "friends," that is, those with a more- or less-compelling call on the government for admission. To this end, he cites a wide range of literature outside of the philosophical field about immigrants' and refugees' rights, including demography, jurisprudence and political science. The book, however, contains relatively little material on present or past immigration or refugee policy or practice. Rather, beginning with the premise that individuals, working through their governments, have moral obligations to admit immigrants, he attempts to develop moral and political bases for differentiating categories and determining responsibilities.

In the first part of the book, chapters 1-5, Gibney takes on the arguments of his fellow philosophical scholars. These chapters cover, sequentially, his interpretations of related works by Michael Walzer, John Rawls, Charles Beitz, Brian Barry, Peter Singer, Henry Shue, and Peter Singer and Bruce Ackerman and his criticisms of their conclusions regarding alien admission. Questioning arguments about admission based on such considerations as family reunification, ideological affinities, and preservation of community, he devotes the second part of the book to elaborating his own principles regarding alien admission.

Essentially, Gibney argues for an alien admission system based on two considerations: first, what he calls the harm principle and second, the basic rights principle. The first would mandate that "states that have caused harm have a special duty of restitution to the victims of this harm" (e.g., on this basis, the United States can be said to owe restitution to citizens of Vietnam, El Salvador, Iran, and Chile). That responsibility includes a willingness to admit those who have been forced to leave as refugees. The second principle contemplates admission—and other forms of assistance—to those with the greatest need, through a "fair share" process among several nations.

Gibney's arguments about which groups should and should not receive priority treatment in an alien admission process are as interesting as they are controversial. Granted, there probably never will be a consensus on what the moral bases of alien admission policies should be, but Gibney presents his points of view compellingly. The book does a service to scholars and policymakers alike in raising difficult questions in this fundamental humanitarian policy area and showing that we need to take a hard look at how they are being decided.

It is unfortunate, however, that so much of Gibney's book—approximately half—is devoted to elaborating the shortcomings of other philosophers' arguments. Gibney's critiques of the works of his colleagues fall short. In the first place, the works he criticizes are not necessarily focused on the same set of problems he wishes to address, although this is not clear from his presentation of their points of view. Second, and related, readers are presented with excerpts that reveal too little about what the various authors wished to posit. Hence, the former cannot really participate in the dialogue unless otherwise familiar with the works under discussion. Finally, and most important, it is Gibney's own arguments, not his lengthy criticisms of approaches that differ from his, that will convince or fail to convince his readers.

PATRICIA WEISS FAGEN

Washington, D.C.

Managing Regulatory Reform: The Reagan Strategy and Its Impact. By Marshall R. Goodman and Margaret T. Wrightson. New York: Praeger, 1987. 236p. $39.95.

Marhsall Goodman and Margaret Wrightson have written a timely and interesting book about the Reagan administration's attempts to control the regulatory bureaucracy and accordingly reform regulation. They use a variety of data in their study, including a survey of a cross-section of federal- and district- and regional-level officials, as well as personal interviews with agency officials, OMB staff, members of congress and their staffs, and interest group representatives. The authors address the following questions: Why did Reagan pursue a management strategy instead of a legislative one? How effective was the management strategy?

The book is divided into seven chapters. Chapter 1, a general introduction to the study's themes, compares Reagan with previous presidents, presents various definitions of what is meant by regulatory reform, and outlines Reagan's management strategy. The authors cogently offer distinctions among different kinds of regulatory reform, including deregulation, devolution, regulatory relief, and rationalizing regulation. Reagan's efforts lay with providing two kinds of relief; deregulation, that is, less regulation and devolution, that is, withdrawal of the federal government from state and local affairs. Little attention was paid to rationalizing regulation. Instead the regulatory policy process was greatly politicized. Reagan's management strategy was built on rescinding rules, slowing down the issuance of new regulations, relaxing enforcement, appointing political loyalists, and cutting operating budgets.

Chapter 2 presents a brief history of regulation, compares the congressional and presidential role in regulatory policy, and discusses the rising tide of antiregulatory sentiment in the 1970s. Chapter 3 employs the survey instrument, finding that Reagan's regulatory attitudes have seeped into the bureaucracy, thereby effectively institutionalizing his program there and perhaps leaving a legacy beyond his administration. Of the interesting findings are comparisons of social and economic regulatory agencies and of independent commissions and executive agencies. Somewhat surprisingly, they find greater presidential than congressional influence over all agencies and, significantly, strong presidential ability to affect the independent commissions.

Chapters 4-6 are well-done case studies of regulatory reform and intergovernmental relations, environmental protection, and nuclear power. Highlighted in each chapter is the strong resistance to the Reagan program, some of it unexpected, for example, state and local resistance to many intergovernmental reform initiatives. Not surprisingly, significant opposition came from Congress and concerned interest groups.

Chapter 7 assesses the strategy and the effectiveness of Reagan's program. While the management strategy aimed to reduce these external influences on regulatory reform, the authors show that plural politics plays a strong role in administrative, as well as legislative, policy-making. Further, the radical nature of the program led to a strong congressional backlash. This is most evident in the environmental protection areas where new legislation increasingly limited bureaucratic discretion. However, one cannot doubt that Reagan effectively changed regulatory policy during his administration. What he did not do was ensure the legacy beyond his administration or rationalize regulatory policy. Doing the first requires legislation; the second requires a less ideological approach to regulation.

In all, this is a sound and well-done study. There are some lapses, however. The authors seem totally unaware of the large literature on political control of regulation by such scholars as Barry Weingast, Terry Moe, and Matthew McCubbins. Hence, they take lack of congressional oversight for congressional ineffectiveness, when it is more properly understood as bureaucratic anticipation and compliance with congressional preferences. Further, they take no note of Moe's important work demonstrating the great impact that presidents have over independent commissions. Hence, the authors are not able to tie their important findings into a more general theory of political control. Such a theory easily explains congressional reaction and activity, especially in the environmental area; the Environmental Protection Agency deviated from congressional intent. However, this is still a useful study and appropriately corrects the literature on the administrative presidency by pointing out the limits of such a strategy.

JEFFREY E. COHEN

University of Illinois at Urbana-Champaign

The Politics of Public Management. By Philip B. Heymann. New Haven: Yale University Press, 1988. 196p. $22.50.

In *The Politics of Public Management,* Philip B. Heymann, a professor of law at Harvard University, draws on his own extensive experience in government and on 16 teaching cases produced at Harvard's John F. Kennedy School of Government to create a perspective on the jobs of the federal government's appointed executives. It is reasonable to argue, he says, that "most decisions about what each government organization will do (and thus what government as a whole will do) are made at the organizational level" (p. 4). Presidential concerns, moreover, "bear on only a relatively small part of the total activities of a department" (p. 73). Thus the appointed heads of these organizations have consequential roles to play and considerable latitude within which to exercise personal judgment. Their effectiveness ultimately depends on their ability "to manage the politics that decides policy within the area of each organization's responsibilities" (p. 8).

The essential task of each appointed executive, in Heymann's view, is to formulate a strategy "to make desirable goals, external support, and organizational capacities fit together" (p. 15). Effective political management lies in continuously adjusting one's aspirations and actions to achieve and maintain the fit. He develops this perspective in a series of chapters analyzing one or more specific cases. Most—for example, experiences of Michael Pertschuk as chairman of the Federal Trade Commission, Joseph Califano as secretary of Health and Human Services, Attorney General Edwin Meese, and Raymond Peck as administrator of the National Highway Traffic Safety Administration—are cautionary tales illustrating the consequences of failure to recognize the subtle demands of inducing an agency to move in a different direction, assembling external support for a controversial policy, or identifying the president's "real" interests. In explaining how these failures might have been avoided, Heymann argues that properly understood, "the management of politics is the pursuit of wholesale and lasting advantages for one's self, one's organization, and one's policies" (p. 142).

The Politics of Public Management is not a work of scholarship. It has neither footnotes nor a bibliography; its recognition of related literature is confined to a single, largely dismissive, paragraph in the preface; and the choice of cases is highly selective. The discussion is loosely structured in a way that suggests it may have originated as a course. The book is rather a thoughtful personal reflection, didactic in purpose and tone. Its value lies in drawing attention to a neglected actor, the appointed executive, and in advancing the notion that by virtue of their knowing more about their departments than anyone else, appointed executives are in a position to make consequential contributions to public policy formation. Had Heymann grounded his argument in ideas drawn from the work of John Kingdon, Murray Edelman, Charles Lindblom, James March, Hugh Heclo, or others who have been concerned with the role of appointed executives or had he drawn on a greater variety of empirical materials, his book might have had value beyond that of supplementary reading.

LAURENCE E. LYNN, JR.

University of Chicago

WPPSS: Who Is to Blame for the WPPSS Disaster? By James Leigland and Robert Lamb. Cambridge: Ballinger, 1986. 253p. $29.95.

Who Is to Blame for the WPPSS Disaster? by James Leigland and Robert Lamb ranks as an important work in public administration not only because it is an excellent study of a complex set of events which captured national headlines but because it analyzes the underlying factors that led to disparate decisions and unanticipated consequences in the management of nuclear power. The authors carefully reveal the historical context of public power in the Pacific Northwest, the formal and informal roles of relevant actors, the political and economic environment in which actors coped, and labyrinthine relationships among actors as they carried out their responsibilities.

The Washington Public Power Supply System (WPPSS) was formed as a municipal corporation in 1957 to acquire, construct, and operate electric power plants for the purpose of the generation and transmission of electricity. Three years after the cancellation of the construction of two nuclear power plants in 1982, the Washington State Supreme Court ruled against take-or-pay contracts. WPPSS shocked its bondholders by admitting it could not fulfill its contractual obligations to them. The repercussions were felt by Wall Street; Washington, DC; municipal bondholders; and electric rate payers. As it was the largest municipal bond default the nation had ever experienced, some explanation was called for.

Even without a default, WPPSS would have been in trouble. The first problem was one of management style, which resulted in an inability of top management to delegate authority to middle and lower levels. As the organization grew in the 1970s, top management became distant from the operating levels it was directing. Secondly, WPPSS top management had little nuclear power expertise, although it was now building nuclear plants. A similar situation occurred at the Long Island Lighting Company (LILCO) as it entered the nuclear power market. Both public (WPPSS) and private (LILCO) utilities showed the same inability to incorporate nuclear expertise into their leadership as they proceeded to develop nuclear power. WPPSS's board of trustees also lacked experience in nuclear power and were incapable of dealing with the overwhelming complex technical issues with which they were confronted. Thirdly, top management engaged in neither strategic planning nor operational planning. This kept information from reaching the top and decisions from being made that addressed overall goals. Lastly, because of Washington state law, which required competitive bidding, resulting in multiple contractors at the five sites, there was duplication of work, no centralized supervision and control, loss of economies of scale, and an absence of standardized design. When WPPSS finally went to the legislature in 1977 to get the law changed, it was too late.

Outside the WPPSS structure, the investment community played an important role. While credit analysts underestimated the ability of participants to pay whether the plants produced electricity or not, they helped WPPSS market their bonds. Moreover, they overestimated demand for power in their forecasts, contributing to a questionable justification for building new plants.

The entrance of rate payers' groups with the attention of the media provided another potent force. Concerned about dramatic increases in their bills, rate payers focused public attention on WPPSS management and were able to stop construction of a plant, pressure WPPSS participants not to pay their debt service, and replace WPPSS board members. Rate payers had reason for concern, as the consortium that financed the Seabrook nuclear plant in New Hampshire has had to pay approximately one-fifth more on electricity, even though the plant has not yet been licensed.

While the book offers a detailed and comprehensive analysis of the WPPSS default, more discussion of the tangled state and federal relationships would have been helpful. The authors point out that greater influence over public authorities such as WPPSS is needed by state governments. Regretfully, public authorities generally have been able to set themselves apart from normal regulatory controls. In an age of nuclear power, that is especially dangerous.

REGINA AXELROD

Adelphi University

Piety and Politics: Evangelicals and Fundamentalists Confront the World. Edited by Richard John Neuhaus and Michael Cromartie. Washington: Ethics and Public Policy Center, 1987. 424p. $23.95 cloth, $12.95 paper.

Political campaigns during the 1980s have focused attention on a newly active interest group in the U.S. electorate, conservative white evangelicals. Political scientists, journalists, and other observers have renewed discussion of the relationship of church and state, as well as examining the goals, aims, and techniques of the Religious New Right. Neuhaus and Cromartie argue that much of this analysis has been shallow and superficial, failing to take into account either the historical roots of this phenomenon or the theological nuances underlying the broad rubric of conservative Protestantism.

This is the breach this volume seeks to fill. In gathering together a wide variety of scholarly work and commentary previously published elsewhere, the editors attempt to shed light on who evangelicals and fundamentalists are, why they have so suddenly come into political prominence, and what different sectors of the evangelical community seek to accomplish in terms of public policy. They largely succeed in this attempt, presenting overall a thorough historical grounding of this strain of U.S. Protestantism. The chapters by James Davison Hunter and George Marsden trace the source of the fundamentalist-modernist split which occurred during the early part of this century, and that by A. James Reichley explains how this split has come to be politically relevant since the 1970s. The general tone of the book is nonjudgmental, neither hailing nor sounding the alarms at the white evangelicals' political advent.

It is divided into three sections. The first consists of generally scholarly work, including the three articles mentioned, introduced by a "Commentary" by coeditor Neuhaus. These four explore the historical development and theological perspectives of evangelicals and fundamentalists, principally focusing on the activity of the Religious New Right. Neuhaus argues that fundamentalists demand a voice in national affairs that they have not enjoyed during a "half-century in the wilderness as objects of deepest contempt" (p. 18), while Hunter and Reichley note that this voice is now possible only after long efforts at developing political resources, as well as the adoption of language that is firmly representative of middle-class values.

This section touches only briefly on black evangelicals, Pentecostals, or radicals such as the Sojourners movement. This becomes problematic in the second section, a collection of essays by prominent evangelicals and fundamentalists. Only two of the 10 pieces are from the Religious New Right; the majority either represent or critique the more liberal-radical message of the Sojourners. In the middle of all this are two more abstract views of political society by James Skillen and Charles Colson. This section is the least coherent in the book, because the groundwork is not laid for understanding the significance of most of these essays.

The third section, consisting of commentaries by "outside observers," is like the first in that it principally focuses on the Religious New Right. Hence, the political significance of the liberals in the second section and their relationship, if any, to their more conservative fellow believers is not clear. A choice of focus for the book—the political activity of all evangelicals or just the burgeoning New Right organizations—was not made.

There are some other unanswered questions. How, for example, do evangelicals relate to charismatics and Pentecostals, their allies in many recent political efforts? If their theological differences affect their politics, a chapter explaining this is vital. Also, where do blacks fit into the picture? Black Christians are for the most part evangelical or Pentecostal yet have not been visible in the new political movements. Is there something racial about the Religious New Right? Unfortunately, the question of racism is not addressed except in passing.

These points aside, this is a fine book. It is worth having for the scholarly articles alone, with several of the later essays serving as valuable documentary references. It would be useful as a reader for a course on religion and politics; brief editorial notes introducing each chapter suggest possible themes and contrasts among different authors. For political analysts who seek better understanding of a recent political phenomenon, this book represents a worthwhile and diverse sampling of research and commentary done on the topic.

BRUCE NESMITH

Western Illinois University

Storm Center: the Supreme Court in American Politics. By David M. O'Brien. New York: W. W. Norton, 1986. 384p. $18.95 cloth, $9.95 paper.

Liberty under Law: The Supreme Court in American Life. By William M. Wiecek. Baltimore: Johns Hopkins University Press, 1988. 226p. $30.00 cloth, $10.95 paper.

The examination of the growth of the power and institutional influence of the Supreme Court of the United States is the central purpose of each of these well-organized books. Both are intelligently conceived, incisive analyses. But they differ fundamentally in organization and approach to the subject.

Liberty under Law is a tightly drawn chronological examination of the origin and development of the Supreme Court's power of judicial review. It's author, William M. Wiecek, has contributed frequently and impressively to the literature of constitutional history with the major works *Equal Justice under Law: Constitutional Development, 1835–1875* and *Nuclear America: Military and Civilian Nuclear Power in the United States, 1940–1980. Storm Center,* by David M. O'Brien, is an excellent analysis of the institutionalization of the Supreme Court mainly in the modern and contemporary era buttressed effectively with occasional historical accounts designed to provide perspective for twentieth-century developments. Both authors bring special competence to their tasks. Wiecek's previous historical projects were of particular importance to his successful integration of the contributions of modern historical revisionism in his concise celebration of the bicentennial of the Constitution of the United States. O'Brien's experience, first as a judicial fellow and then as a research associate in the office of the administrative assistant to the chief justice of the United States, was particularly valuable for his excellent chapters on the internal operation of the Supreme Court.

In contrast to David Currie's attempt at evaluating each eighteenth- and nineteenth-century Supreme Court cohort in accordance with current canons of judicial restraint, Wiecek avoids imposing ex post facto criteria. He prefers instead to treat the major doctrinal developments of each major historical era in accordance with the standards employed by the contempories. Thus John Marshall's judicial activism in *Marbury* v. *Madison* was not only criticized as an enlargement of judicial authority but, according to Thomas Jefferson (and later Andrew Jackson), as judicial aggrandizement violative of a concept of separation of powers that envisaged coequal responsibility for constitutional interpretation by each of the three branches of the national government. *Liberty under Law* comprises a brief introduction and eight chapters. The introduction employs James Bradley Thayer's late-nineteenth-century essay on the origin and scope of the U.S. doctrine of constitutional law, focusing upon the issue of the legitimacy of a nonelected judicial elite's assertion of the power to determine the scope and limitations of the powers of the elected branches definitively. Chapter 1 examines the evidence for and against judicial review from the perspective of British colonialism, U.S. revolutionary theory, immediate postrevolutionary experience, and the first decade under the Constitution of 1787. In chapter 2, the broad and often-enduring impact of Chief Justice John Marshall is sensitively explored. Chapter 3 discusses the Taney Court's decisions involving capitalism, democracy, and slavery while chapter 4 investigates decisions involving the Civil War, Reconstruction, and the immediate postwar rights of black people. Chapter 5 chronicles the dismal record of Supreme Court retreat from support for minority rights and, concomitantly, its expansion of corporate power and protection from regulation in the public interest. Chapter 6 deals with the Court's response to foreign affairs issues and the expansion of executive power. Chapters 7 and 8 examine substantive equal protection and substantive due process, respectively. The last eight pages of Chapter 8 are devoted to an analysis of the Court's contemporary critics, concluding that "the coherence of constitutional theory disintegrated in the 1980s, partly because of a breakdown in agreement on the fundamental terms of the debate" (p. 190).

Storm Center contends that the Supreme Court has ceased to be "the least dangerous branch" and, in the modern United States, has become a "storm center" because decisions such as *Roe* v. *Wade* and *Brown* v. *Board of Education* have stimulated significant social and legal change. But David O'Brien does not argue that *Roe* v. *Brown* substantiate the dire eighteenth-century critique of the Supreme Court proposed in 1787, in Robert Yates's *Letters of Brutus.* Indeed, he acknowledges that the Court has always been a center of controversy because, as Toqueville perceptively observed, many divisive political issues become major legal and constitutional issues. Thus O'Brien seeks to combine major emphasis upon the Supreme Court's significant influence on U.S. life as well as law with a thorough examination of the manner in which the Supreme Court conducts its institutional tasks. In chapter 1, "A Struggle for Power," the issue of abortion is examined primarily from the perspective of internal Court response. Chapter 2 reviews the selection process, off-the-bench activities of the justices, and the twin principles of judicial independence and accountability. The remaining four chapters provide very well researched examinations of the institutionalization of the Court in the twentieth-century (chap. 3), the internal issues and process of deciding who shall have access and how (chap. 4), the decision-making process itself (chap. 5), and the recent changes in the manner of presenting opinions to the profession and the public (chap. 6). Unlike *The Brethren* (1979), O'Brien's use of information from within the Court and its officials is carefully documented and, most importantly, is employed to illuminate and make more understandable the internal processes. The author accomplishes this in a well-balanced series of analyses.

Each book provides a comprehensive index, a glossary, and a selection of publications for further reading. O'Brien also includes an appendix containing the chief justices and associate justices, their appointing presidents, and the dates of their service on the Supreme Court of the United States.

JOHN R. SCHMIDHAUSER

University of Southern California

The Politics of Federal Reorganization: Creating the U.S. Department of Education. By Beryl A. Radin and Willis D. Hawley. New York: Pergamon, 1988. 253p. $29.51 cloth, $12.91 paper.

Agency Merger and Bureaucratic Redesign. By Karen M. Hult. Pittsburgh: University of Pittsburgh Press, 1987. 221p. $28.95.

Reorganize a bureaucracy, and one never knows whether, after all, something new and splendid or something old and dreary will result. Best hopes usually end up as disappointments. But even where success seems apparent, closer inspection can yield a mixed message with plenty of qualifications. Both books bear witness here.

Nonetheless, rearranging boxes on an organization chart appeals to politician and citizen alike, as each seeks usually unattainable and nearly always simple-minded changes in institutions doing the public's business. Improved control, greater efficiency, more accountability, less redundancy, and a host of other rationalizations exist and fuel desires to fix bureaucracies. Sometimes the rationales fit and make sense. Often they do not. But throughout, one truth about bureaucracies stands out: how hard it is to make a dent in them, much less make them over. Again, both books provide ample testimony, although the approaches taken and the specific messages delivered are different.

Radin and Hawley, both experienced scholars with an active practical bent, consider the creation of the U.S. Department of Education during the Carter administration. They organize their inquiry according to the respective steps taken along the way from initial political promises to the birth of a "new" cabinet office in October 1979. That this experience counts more as a reorganization than a genuine creation comes through clearly as a core theme and a main reason why the Education Department turned out as it did. Throughout the process the authors introduce and weave key personalities and events together in a rich and believable human tapestry. Their job is informed by several different bodies of organizational thought, including processive, administrative, cultural, and sociological ones. They emphasize, however, a crisp recitation of the case itself and so present many specific matters of fact and several general lessons that bear repeating.

Creating (reorganizing) something as complex as the Education Department outstripped any analyst's capacity to imagine beforehand what would eventually happen. False hopes, inflated claims, and plain mischief hardly helped matters but did conspire to challenge and frustrate the analysts involved. Humility, or at least lowered sights, is indicated. The point holds at least as forcefully for would-be reorganizers. Expect less and keep the means simple to avoid disappointment and to minimize errors. Be prepared to adapt and improvize rather than holding to a rigid plan or grandiose scheme.

One person's creation is often someone else's destruction. The case is fruitfully imagined as being a matter of termination, at least for many actively involved in it. Thus one might begin to see and appreciate better much of the opposition and many of the unexpected twists and turns the Education Department decision and process took. This viewpoint stresses powerful ideas the termination experience can evoke, not the least of which is the connection between act completion and problem generation.

Hult's book features comparisons of three different institutions, all of which were intentionally merged. Another common denominator is a focus on community development, but taken from different levels of governance: the U.S. Department of Housing and Urban Development; the Minnesota Department of Energy, Planning, and Development; and the Minneapolis Community Development Agency.

Despite differences among her cases and between Hult's work and that of Radin and Hawley, strong similarities in message and finding stand out.

First, contextual details matter very much in each instance. Indeed, unique and specific facts overwhelm various theories meant both to forecast and explain processes and events in a more general way. In this regard Radin and Hawley's selective experience proves no more helpful than Hult's expansive efforts to cite and test many different theoretical schemes. (The book's origin as a dissertation jumps out here and elsewhere.)

Second, individuals with vested interests know reasonably well and quickly how proposed changes will affect their lives. Such knowledge motivates them mightily. The reformer's pristine scheme for redesign or renewal is soon sullied, if not brutally violated, by many who apprehend the benefits of an imperfect status quo and care not at all to pay for an illusory perfect future.

Third, grappling with and trying to change our institutions is hardly likely to pass from vogue. Indeed, the luxury of starting from scratch, with its resulting bloat in government, is increasingly rare. A demand to recycle and redirect human organizations, rather than allowing them to multiply and grow, stands out. And to the extent it does, such books as these become useful for the experiences, insights, and wisdom they convey.

A note on the books, as books, is in order. Both are well organized and written. They are short, nearly always on point, well-documented, and easy to take. And, remarkably, an editor's helpful hand is evident in each. They are in plain English, not impenetrable jargon, and so ought to be accessible to many different audiences.

In short, these are solid contributions, have valuable lessons for scholars and practitioners alike, and deserve notice as considerably more than "just a couple of case studies."

GARRY D. BREWER

Yale University

Elections American Style. Edited by A. James Reichley. Washington: Brookings Institution, 1988. 291p. $32.95 cloth, $11.95 paper.

The title of this book raises expectations of a comparative theme, or at least some discussion of what constitutes an "American" style of elections. In fact, the focus is much narrower. Specifically the book concerns various institutional limitations in the current election system and process of the United States.

Two basic assumptions underlie the arguments presented in this book. The first is that the U.S. electoral system is in trouble. The second is that as the electoral process changes or is changed, so, too, do the outcomes. Proposed reforms in the electoral system should therefore produce desired and prescribed improvements.

The chapters of this book, derived from a Brookings conference, examine several major criticisms of the current electoral system and seek to determine their validity and, where necessary, prescribe remedies. As viewed from within the Washington beltway, the problems currently plaguing the electoral system include the huge cost and long duration of campaigns, which are distorting and corrupting the system; the power of the media to shape public perceptions of candidates; the undue influence exerted by "special interests" over both nominations and general elections; declining turnout, which is ascribed variously to restrictive electoral regulations, enlargement of the eligible electorate, and profound alienation; as well as the weakness of the political parties, which increases reliance on special interests for support and undercuts the capacity of the federal government to function on behalf of a broader "national interest."

Four chapters in particular do an outstanding job of examining the argument connecting change in electoral process and procedures with change in electoral outcomes. The first of these is the chapter by Eddie N. Williams and Milton D. Morris, which reminds us that historically the single most important and persistent problem of the U.S. electoral process has been that of inequitable access. But because of enforcing the 1965 voting rights act, the participation levels of whites and blacks have converged. Nevertheless, there are still racially motivated problems that persist, especially in the drawing of electoral district boundaries and run-off rules.

One fascinating discussion in the Williams and Morris chapter explores the erosion in the basis of party support for the Democratic party that has resulted from the adoption of the proportionality principle ensuring that the delegates at the convention reflect the racial, ethnic, and gender composition of the party's voters. They argue, quite convincingly, that this contributed to the loss of support for the Democratic party among white male southerners.

Another influence in the electoral process that has clearly evolved from changes in rules and regulations is political action committee (PAC) spending. Larry Sabato, in an excellent chapter on corruption in campaign financing, argues against further so-called reforms, including putting limitations on the size of PAC spending. He contends that such limitations would unfairly benefit incumbents and wealthy candidates and that it would increase independent expenditures, that is, expenditures by groups that are not connected with or controlled by any candidate or party, for example, the National Political Action Committee. He does, however, argue that certain of campaign finance laws make sense. In particular, he argues that political parties should be given preferential treatment in campaign finance laws so as to strengthen the parties relative to the PACs.

The chapters by Kay Lawson and Thomas Mann are also especially pertinent to the thesis of the book. Lawson effectively argues that state laws make it difficult and sometimes impossible for parties to form and to get on the ballot, to control their own nomination processes, to define issues effectively, and to hold their elected representatives accountable. Thomas Mann convincingly contends that redistricting congressional boundaries over the years has not protected incumbents or prevented the Republicans from becoming the majority party.

All the chapters from this book make cogent, reasonable arguments pointing to problems in the electoral system, some of which, their authors argue, may be affected through institutional change. The difficulties here arise less out of the definition of what is a "problem"—although the book fails to address that concern—than from the all-too-brief discussion of criteria for judging prescribed changes, as well as the lack of empirical foundation for the many arguments presented. If we are to conclude that there are problems or prescribe remedies, we need criteria for judging how well the system is functioning. Some of the recommended changes in this book would reduce the extent to which the system is democratic, for example, returning to party-controlled conventions in place of primaries as the means for nominating presidential candidates. Certainly, many people, including some prominant political leaders such as Jesse Jackson, would find this an unacceptable alternative to the current nomination process. This is not to suggest that the current procedures are perfect, only that there are numerous perspectives on these issues. The arguments and prescriptions presented in this book are quite provocative and should be of interest to anyone specializing in the study of the U.S. electoral system.

ARTHUR H. MILLER

University of Iowa

Privatization: The Key to Better Government. By E. S. Savas. Chatham, NJ: Chatham House, 1987. 308p. $14.95.

Although there is much about this book that is annoying, *Privatization: The Key to Better Government* contains a very useful introduction to the rationale, methods, and purposes of privatizing government services. By *privatization*, Savas means "the act of reducing the role of government, or increasing the role of the private sector, in an activity or in the ownership of assets" (p. 3). Savas tries, sometimes with success, to maintain a scholarly and analytical stance on his subject, but he often fails to hold in check his predilection towards advocacy and, unfortunately, indulges too often in cheap shot criticism of government.

Because of his strong ideological attachment to privatization as a principle, Savas is prone to exaggeration. For example, he claims that the growth of government endangers the strength of other social institutions and limits social diversity, as well as risking excessive dependence on government. Actually, in modern, capitalist systems it is clearly private forces that have done the most to homogenize culture, to create national markets, and to undermine local diversity. Savas seems completely ignorant of the virulent manner in which private markets operate to dissolve local tradition and culture. In various places, Savas even indulges in one of the more specious claims of the political Right, that government is responsible for undermining the family.

Yet the private market's pressures of work and the quest for material gain seem to place far greater stress on families than anything the government may be doing.

Nevertheless, tucked in the pamphleteering, Savas does manage to provide a number of chapters that comprise perhaps the most lucid introduction to the concept of privatization available. The book is organized into four parts. The first part involves a discussion of the factors that have given rise to the privatization movement. Savas claims there are four "pressures" for privatization that have emerged in recent years, including a *pragmatic* desire to have things work better than before, an *ideological* interest in reducing the role of government, a *commercial* desire among private sector actors to benefit more from government spending, and a *populist* interest in the public's having greater choice in the production and distribution of public goods and services. Moreover, Savas also claims that the sheer size and burden of government activity in the United States has accentuated the previously mentioned pressures for privatization.

Although there are commercial and populist reasons for the emergence of the privatization movement, clearly the most important is ideological, with some impetus provided among fiscally stressed states and localities attempting to garner savings by contracting out public services. Savas tries very hard to imply that the growth and burden of government has fostered the privatization movement, by exaggerating public sector burdens and downplaying the fact that the public sector in the United States is among the smallest in terms of tax burden or percentage of GNP. In fact per capita government revenues as a ratio to per capita income has changed very little in the past 15 years. Savas, in short, seems to want to obscure the close connection of privatization with conservative ideas, although such a connection might not be a logical necessity. As Savas correctly points out numerous times, an active, interventionist government can, in theory, still rely on privatized methods to achieve public goals.

The second part of the book contains the two best chapters. Savas introduces the reader to the theory of public goods and services in a lucid, jargon-free manner. Moreover, Savas demonstrates nicely how concepts are analyzed for the purpose of developing a classification of goods and services. He then links the various types of goods and services (private, common pool, toll, and collective) to alternative methods of providing such services. These methods are functions of the class of good and service—whether the government or private sector arranges for the good or service—and of who produces the good and service. The result is 10 categories of institutional arrangements for producing and delivering goods and services, based on whether it is the government or the private sector that is arranging and producing goods and services. Savas then does an excellent job in describing the range of activities to which different institutional arrangements can be extended, especially in the sense of extending the role of private markets and reducing the role of direct government provision.

The second part of the book ends with a chapter analyzing and comparing the various arrangements to provide goods and services. In this section Savas provides a reasonable—although by no means exhaustive—set of criteria for evaluating the production of goods and services. The difficulty is again a kind of stacking of the deck, in which the methods that emphasize the private sector seem to manifest fewer of the problems than do those that involve government more directly. Savas is correct in pointing out that in some contexts and for some services privatization will produce more honesty, responsiveness, accountability, effectiveness, and equity. Yet he cannot leave well enough alone. The result is that the strongest section of the book again ends sounding like a partisan brief for privatization.

The third part of the book is in some ways the most difficult. On the one hand, it is very important to explore the range of activity to which various forms of privatization can be extended. Since the extent and nature of the mix between public and private producers and arrangers of goods and services are so plentiful, the conceptual application of the methods of privatization are equally numerous. Savas does a very good job giving the reader a picture of the vast range of activity to which privatization can be extended, ranging across physical-commercial services (solid waste management, transportation, and management of government property) and protective-human services (public safety, education, national defense, housing, and social welfare). Yet as interesting as this discussion of privatization opportunities is, much of it is overstated and more in the nature of wishful thinking.

The final section of the book concentrates on the issue of achieving privatization. Savas reviews the political feasibility of various methods of reducing the direct role of government provision and distribution of goods and services. He recognizes that greater privatization is "more a *political* than an *economic* act" (p. 233). The concluding section then purports to discuss the problems with privatization. But rather than discussing the problems of privatization, Savas discusses the barriers to achieving privatization. The reader who is an agnostic on the question of privatization, rather than an ideologue, cannot fail to end the book with a feeling that for Savas there are no bounds on privatization other than political. For Savas there seems far less interest in a careful, sensitive effort to place privatization into a scholarly context in which the suitability of the various privatization efforts can be understood. Savas seems quite willing to dismiss as sheer partisanship concern by many about the long-term implications of a massive privatization campaign of the kind he so clearly prefers.

I found this book well worth reading. I believe others will gain too, although a more circumspect approach by the author would have made the work even more useful.

MAX NEIMAN

University of California, Riverside

The Structure of Power in America: The Corporate Elite As a Ruling Class. Edited by Michael Schwartz. New York: Holmes & Meier, 1987. 256p. $39.50 cloth, $22.50 paper.

The pluralist view is that business groups do not constitute a cohesive elite but tend to cancel each other out with their countervailing demands. Furthermore, they are checked by other nonbusiness interest groups and by the democratic rules of the game. A contrary view offered in this collection of scholarly essays by 14 social scientists is that the giant corporations are by far the most powerful of all interests in society. They prevail, if not omnipotently, then preeminently. Furthermore, they are not floating free in social space but are linked to each other by common classwide interests.

This book is heartily recommended to those political scientists who have spent decades writing about the presidency or Congress or other aspects of U.S. government without ever finding it necessary to mention capitalism and without ever dealing with the imposing presence of the moneyed power. In their respective contributions to this volume, Mark Mizruchi, Beth Mintz, Michael Schwartz, James Beardin, Donald Palmer, and Michael Useem demonstrate that contrary to the Berle-Means thesis, profit considerations are first and foremost among corporate leaders, most corporate stock is owned by other corporations including banks and insurance companies, and there is an increasing degree of intercorporate stockholding and directorships (including bank directorships).

The picture is of a great concentration of wealth, control, and decision making. The dominant force is not industrial capital but finance capital, with bankers and officers of other lending institutions so in control of nonfinancial directorships and, more importantly, of capital flow as to be able often to determine a firm's investment decisions. The authors provide us with a well-illustrated, X-ray examination of how overlapping top corporate officers compose an "inner circle" in an otherwise seemingly diverse business community, welded together by both company relationships and by kinship, elite education, and social club membership. This inner circle tends to be highly cognizant of the imperatives of the capitalist system as a whole. Rather than relying on a cohesive and formal interorganization, these elites have created "a loose but nonetheless very real system of coordination in which firms are instruments of inner circle policy" (p. 70).

These business elites have understood the difference between federal regulation *of* business and federal regulation *for* business, as Tom Koenig notes in his essay. They learned how to use government regulations to gain advantages over smaller competitors and to shift costs onto the public. They are unmatched by any other private group in their ability to make serviceable things happen in the private and public realms, regardless of which party occupies the White House. As contributors Laura Ankev, Peter Seybold, and Michael Schwartz note, business leaders establish their own policy-planning groups, whose members regularly staff the top government posts and whose proposals are usually translated into public policy. Corporate leaders also serve on government advisory commissions, helping to finalize and oversee government operations. And they dominate the boards of directors of public institutions.

But might not business leaders be serving not themselves—or at least not only themselves—but the entire community? Judging from the book's essays by J. Allen Whitt, David Gersh, Richard Ratcliff, and Peter Seybold—who investigate such areas as education, philanthropy, transportation, community development, and even the development of IQ testing and the financing of behavioralism in political science—business watches out for business. And business people use the land, labor, capital, technology, and markets of our society at a real cost to us as workers, taxpayers, consumers, and community residents. Business elites may not always consciously conspire with long-range plans, but they generally know which policies serve and which threaten their class interests.

The concluding essay by Dan Clawson and Mary Ann Clawson on the reemergence of the "new conservatism" is excellent, the best in the book. It credits Reagan's unprecedented success in implementing his conservative agenda not to his charisma (his most successful years, 1981–82, were his least popular) but to the decision by business to roll back the social democratic gains of the previous two decades, the better to safeguard the business system from the ever-troublesome and sometimes dangerous encroachments of popular agendas. What is called "Reaganism" was first introduced by Carter as early as 1978 and was a conservative way of resolving the conflict between capitalism and democracy.

Michael Schwartz is to be congratulated for putting together an exceptionally well integrated and well edited book and for his own substantial essay contributions. Still, I would take issue with him on one point. In his preface he makes the astonishing assertion that both "instrumentalist" and "structuralist" Marxists "assume that government is the only source of policy in society. . . . They leave aside the set of interrelationships between government and business" (p. x). I know no Marxist who takes that position. Schwartz defines structuralism as government structures "congealed into organizational processes" and acting as the overriding determinants of policy (p. ix). In fact, structuralism is something else, namely the view that the *structural constraints of capitalism* act as an independent force on political officeholders, who have to deal not merely with "the economy" but with a capitalist economy—and on its own terms. Schwartz and other contributors correctly conclude that structure is not automatically self-generating, no matter what the systemic imperatives. The conditions of hegemony must constantly be recreated through human agency. This book does a fine job of describing some essential features of that process.

MICHAEL PARENTI

Howard University

Voter Mobilization and the Politics of Race: The South and Universal Suffrage, 1952–1984. By Harold W. Stanley. New York: Praeger, 1987. 185p. $35.00.

Black voter registration and turnout increased dramatically in the South during the period following *Smith* v. *Allwright*. With the passage of the Voting Rights Act of 1965, the last major legal barriers to widespread black voter participation were eliminated. Black registration rose by 33%—29% to 62%—during the 1960–70 decade. Thus most of the research on southern electoral expansion focuses on the expansion of the black electorate. Stanley's *Voter Mobilization and the Politics of Race: The South and Universal Suffrage, 1952–1984* examines both black and white expansion with the goal of answering two critical questions: (1) Why did the expansion occur? and (2) what difference has it made?

This expansion is especially noteworthy since the nation experienced an overall decline in national turnout while the South experienced a steady increase. This volume challenges the conventional scholarly wisdom regarding why the expansion occurred. Although heretofore scholars had not systematically studied the southern expansion, the reasonable assumers argued that two phenomena occurred: blacks increased their political participation and out-registered blacks in absolute number almost five to one between 1960 and 1980. Maintaining that the countermobilization theory is based largely on assumptions and intuition rather than supporting evidence (p. 8), Stanley seeks to examine the evidence for the racial countermobilization theory.

Stanley doubts that racial countermobilization constitutes the principal reason for the white electoral expansion, since the South has been affected by a variety of political, social and economic changes since World War II. Thus Stanley uses the multivariate logit to explore the relative importance of several clusters of costs and benefit variables: (1) racial attitudes, (2) socioeconomic characteristics, (3) enhanced political competition, (4) shifts in political attitudes, (5) greater media usage, and (6) easier registration requirements. Although these variables had varying impact on the black and white electorate, the author's crucial finding is that racial attitudes ranked lowest among the variable for blacks and whites (p. 43). His analysis confirms his hypothesis that other factors, primarily those associated with the costs and benefits clusters, are the primary reasons why the South experienced an electoral expansion. He then proceeds to analyze the difference that this expansion has made.

Stanley's section on the impact of the electoral expansion is interesting and enlightening. He finds that while there are regional implications for the electoral expansion, the impact of the electoral expansion can be affected by other factors such as the organizational effectiveness of the new voters and the response that they engender from the political elite. The regional implications include a new southern political culture characterized by more liberal and progressive voters.

Much of Stanley's case against the countermobilization theory rest on the conclusion that the survey data clearly indicate that the racial attitudes of the new white southern voters simply do not conform to a mind-set that would support black suppression (pp. 45, 47). This reasoning implies that since the new voters did not appear to be unfavorably disposed to blacks, these voters were not mobilized in an effort to thwart black progress. But the fact is that one can be mobilized for a cause yet oppose that cause as a matter of principle. Why and how one votes is vastly different from why one was mobilized to vote. The author's failure to take this into account constitutes a weakness in the analysis.

Stanley further contends that while "racial backlash does not account for a sizeable share of the white turnout in the region, such backlash did occur on occasion" (p. 50). He uses examples of the George Wallace countermobilization effort in 1966 and the countermobilization effort centered around Charles Evers's gubernatorial campaign. These cases are well documented. However, other less-documented cases exist, especially in the rural black belt South.

It is interesting to note that Stanley does not use the attitudes of the voters here to verify the existence of a countermobilization effort. The existence of a well-organized campaign suffices as evidence. Thus he introduces a new measure of countermobilization that flies in the face of the other measure, racial attitudes. If countermobilization is to be measured by the existence of campaigns to increase the white vote, it follows that perhaps countermobilization is a larger factor in white turnout than the multivariate logit analysis would indicate.

Stanley does a fine job of analyzing the consequences of the electoral expansion in the South. He strikes a delicate balance between a discussion of the accomplishments and the limitations of the expansion. While most of the literature on the "New South" focuses on the impact of the black electorate and their success, Stanley goes further and gives some much-needed attention to the limitations. His discussion on the role of organizational effectiveness and black economic dependence is especially informative.

In spite of the reservation regarding the measurement of countermobilization, *Voter Mobilization and the Politics of Race* makes a valuable contribution to the literature on political mobilization. Stanley presents a good case to support the argument that many factors are responsible for the southern electoral expansion. Scholars of political parties and elections, southern politics, black politics, civil rights, and state and local government have another informative and well-written volume to explore. This book is well worth the reading.

LAWRENCE J. HANKS

Tuskegee University

Policy Paradox and Political Reason. By Deborah A. Stone. Glenview, IL: Scott, Foresman and Little, Brown, 1988. 316p. $10.50 paper.

A few rare books intended for students and the textbook market also have something important to teach the academic professional. This is such a book. It is clearly meant to be used as a textbook in undergraduate, and possibly graduate, public policy courses. It does, however, provide a number of insights into the politics of policy-making—as well as the contents of the policies that emerge from the process—that would be informative for most scholars. The book should be read by students of policy in political science, and, even more, by scholars in other disciplines. It would be valuable to our colleagues in other disciplines because it explicates very clearly a political (and almost social constructionist) perspective on policy. In this analysis policy-making proceeds by strategically creating paradoxical situations and then resolving them in desired manners. Such an approach is hardly the stuff of most "rational analysis," and it demonstrates the incompleteness of that approach for understanding what actually happens in decision making in the public sector. Rational analysis is not seen as wrong but rather as only a part (a small part) of the analysis that must be undertaken to understand policy.

Although it finds rational analysis incomplete and potentially misleading if used in isolation, this does not imply that this book itself is unsystematic. Instead, the author imposes a tripartite breakdown to understand the elements of public policy: goals, problems, and solutions. One may quibble over whether the subcomponents (essentially the chapters) of this scheme are collectively exhaustive (or are at the same level of generality). The basic framework, however, does point to the need for public policy analysis to contain some understanding of the situation the policy addresses, a set of goals that it intends to reach, and the set of instruments employed to reach the goals. Too often one or more of these elements of analysis is taken as a given and not subjected to the rigorous examination it requires. For example, efficiency is often posited as *the* goal for policy-making, although Deborah Stone demonstrates clearly the multiple and often conflicting values which any policy touches. Further, unlike efficiency, there is almost no hope of providing neat, quantifiable indicators of the achievement of most policy goals; policymaking is much less tidy than many analysts would have us believe.

While all three sections of the analysis have substantial interest for a reader, the third section—on solutions—is at once the most interesting and somewhat exasperating. The issue of the "tools" that government employs to attempt to achieve its policy goals has received an increased amount of scholarly concern over the past several years. Scholars such as Hood, Elmore, Salamon, Mosher, Kettl, Atkinson and others have advanced schemes for classifying these tools. Stone offers yet another scheme, albeit one with a somewhat more political flavor than most of the others. Further, unlike many of the other scholars mentioned, she does make an attempt to evaluate the relative utility of the various alternatives and to point to trade-offs of values that they imply.

What is mildly exasperating, however, is that this is yet another taxonomy of tools without much of a theoretical or empirical base. It would appear that progress in the understanding of these instruments will be made not by advancing still more schemes (no matter how interesting) but in relating classifications to a theoretical base. Further, greater care should be taken in the (usually implicit) linkages between problems and solutions. This has been done casually and partially in several places, but has yet to be done systematically. (A major exception may be Kirschen's explication of the numerous tools of economic policy.) A book written primarily as a text may not be the most appropriate place for such a theoretical effort, except that the author has done such an excellent job in addressing other issues with substantial theoretical content in a comprehensible and readable manner. This mild complaint, however, may reflect my own agenda for policy research rather than any serious deficiency in the book.

In summary, this is a valuable contribution to the literature on public policy-making. It points to the central role of politics in the definition and solution of policy problems, a role that is difficult or impossible to capture in mere rational terms. This is, however, more than the defense of a discipline. It is a reasoned and insightful analysis of public policy and the long and difficult process by which it is made.

B. GUY PETERS

University of Pittsburgh

Public Utilities and the Poor: Rights and Responsibilities. By David C. Sweet and Kathryn Wertheim Hexter. New York: Praeger, 1987. 174p. $37.95.

Public Choice and Regulation: A View from Inside the Federal Trade Commission. Edited by Robert J. Mackay, James C. Miller III, and Bruce Yandel. Stanford: Hoover Institution Press, 1987. 363p. $41.95 cloth, $22.95 paper.

No two books on regulation could be further apart ideologically. Sweet and Hexter advocate more regulation to assist the poor. The arguments of Mackay, Miller, and Yandel are antiregulation; the poor would be better served by the market than by all the sympathetic regulators in Washington.

Sweet and Hexter examine post–energy crisis policies to help poor people cope with increased energy costs. Of the three basic policy options of conservation, financial assistance, and rate reform, they argue most policies focused on financial assistance. Sweet and Hexter characterize this as a "short term approach to a complex problem" that does little to solve underlying problems. After discussing various energy-related policies, Sweet and Hexter predict that access to telephone service will be the next major equity issue for public utilities.

Public Utilities and the Poor is essentially a descriptive overview of potential policy reforms. While much original data are gathered, little analysis is done. An excellent multivariate study of public policies could have been conducted with the information the authors gathered. As a result, political scientists might glean a lecture from the book but not much more.

In *Public Choice and Regulation* the authors live the ultimate academic fantasy. Most of the authors are academic economists employed by the Federal Trade Commission (FTC) during the Reagan years. This access enables them to gather data and analyze FTC policies from a public choice perspective. Exactly what a "public choice" perspective is is unclear from the readings although the editors define it as studying the "systematic influence on the agency's behavior of external factors—such as Congress, the executive branch, and interest groups—and internal factors—such as organizational structure and incentives" (p. 4). How public choice differs from traditional bureaucratic politics is unclear.

Fourteen empirical chapters primarily on the FTC's antitrust policies (consumer protection is virtually ignored) are the core of the book. The authors take to heart George Stigler's effort to persuade economists to abandon normative market failure explanations of regulation in favor of his empirical "capture" theory. The book is a must reading for students of regulation simply because it represents the cutting edge of empirical economics as applied to political problems. The chapters by William Kovacic on the history of the FTC, by William Shughart and Robert Tollison on antitrust recidivism, and by Robert Rogowsky on mergers are quality studies.

The studies constitute a great graduate-level text, in part because they represent the economics version of political economy at its best and its worst. Clearly the greatest shortcoming is methodological. Since political scientists are frequently criticized by their analytically superior economics brethren, such a failure is surprising. Measurement problems abound. Most chapters treat all FTC cases as equal and perform complex statistical techniques with aggregate caseloads. By making no qualitative distinctions, they ignore the major criticism of the FTC, its reliance on trivial, easy-to-win cases. The authors need to heed Kovacic's study as he documents Congress's concern with the quality rather than the quantity of cases (pp. 85–86).

Inappropriate measurement is only one problem. Data that might not fit the model are simply tossed away; Langenfeld and Rogowsky, for example, delete one-third of all merger cases without any justification because they deal with banks (p. 212). The impact of policy interventions are misspecified; the impact of the FTC advertising substantiation rule is measured as a dummy variable from the time it was proposed until the time it was adopted rather than for the entire duration of its existence (p. 192). Some findings are nothing more than measurement artifacts. Altrogge and Shughart's finding that antitrust penalties are regressive (pp. 243–44) is a function of a ceiling effect on the size of penalty. Although many of the models are highly collinear (as one would expect if Americans for Democratic Action scores for Philip Hart and Peter Rodino were both independent variables), no concern is expressed. Ordinary least squares estimates of time series regressions often have too much autocorrelation (p. 151), while other authors proceed directly to generalized least squares estimations without checking for autocorrelation first. The level of statistical significance is ignored if results are consistent with the model (p. 149).

Next to methodological limits, the most serious failing is the lack of citations to relevant political science literature (Bruce Yandle is the one exception). Directly relevant empirical work on regulation by Terry Moe, Jeff Cohen, and Dan Wood is not cited. Neither is Barry Mitnik's agency theory approach, despite a heavy reliance on agency theory. The chapter on FTC chairs would be greatly improved by use of David Welborn's work, and the analysis of FTC commissioners' voting behavior by Ronald Bond and James Miller is primitive compared to the work in political science. Perhaps the most significant omission concerns Barry Weingast and Mark Moran's previously published work on congressional dominance of the FTC. That a volume would publish this work without even a passing reference to the devastating critique of congressional dominance by Terry Moe raises serious questions.

The empirical work on the FTC would be greatly helped by a good concluding chapter. Such a chapter would resolve the conflicts between individual chapters (e.g., Weingast and Moran conclude the Senate is the most powerful influence on the FTC while Faith, Leavens, and Tollison conclude it has little impact; different authors advocate both more and less antitrust). It would also discuss public choice's strengths and weaknesses and areas that need more research or greater theoretical focus. Such a chapter would showcase the approach and save the book from any errors by individual authors. Instead the book concludes with a meandering essay by Gordon Tullock who ignores the research in the book and jumps from one unsubstantiated generalization to another.

No short review can do justice to 14 empirical studies of the FTC. Despite the limitations, the book should be read by all scholars of public policy. The book will be highly influential in economics and among some public choice political scientists.

KENNETH J. MEIER

University of Wisconsin, Madison

The American Perception of Class. By Reeve Vanneman and Lynn Weber Cannon. Philadelphia: Temple University Press, 1987. 363p. $29.95.

The long and persistent controversy, from German sociologist Werner Sombert and Frederick Jackson Turner to Louis Hartz and Seymour Martin Lipset, on why the United States failed to develop a Socialist party and a strong labor movement has been revitalized by Reeve Vanneman's and Lynn Cannon's study. The authors discount all previous explanations of U.S. exceptionalism because they are based on the erroneous assumption that U.S. workers are not class-conscious. Vanneman and Cannon readily admit that when judged by their actions, U.S. workers have generally lacked militancy and class consciousness. But to judge workers' actual state of mind on the basis of their behavior is to be guilty, they argue, of psychological reductionism. To avoid this error, they set themselves the task to investigate the degree to which various segments of the working class actually perceive society in class terms.

Following the lead of Nicos Poulantzas, power in the form of ownership, authority, and mental labor is seen as the determinate of class divisions. Within this power context, members of the working class are considered to be persons in the workplace who neither exercise authority over others nor participate in the design of work nor work for themselves. Conceptualized in this way, approximately 70% of all working U.S. citizens (comprised of the unskilled, skilled, and lower-white-collar) are working class. Class-conscious members of this class are defined as those who perceive a class division in society that separates them as workers and subordinates to the middle class (professional, state officials, etc., who share, directly or indirectly, in the control of workers' lives) and managers and corporate executives.

Based on an analysis of the survey studies of the U.S. working class, together with a modest but methodologically sound survey of their own on how U.S. workers actually think, Vanneman and Cannon conclude that a predominate number of this class are class-conscious.

In an interesting chapter comparing working-class consciousness in the United States and Britain, the data indicate that contrary to the usual assumption, British workers are no more class-conscious than their U.S. counterparts. Britons do identify themselves as members of the working class more than U.S. citizens. However, this reflects a larger middle class in the United States than in Britain rather than a difference in class consciousness of workers in the two countries. When U.S. and British workers in roughly equivalent positions are compared, they are equally likely to label themselves working class. Along somewhat the same lines, the authors explain that more British workers vote their class position than U.S. workers, due largely to the fact that U.S. citizens do not have the political choice between middle-class and working-class parties. In adopting Walter Dean Burnham's argument, Vanneman and Cannon point out that the 'real' class struggle acted out in U.S. elections is not between Republicans and Democrats who vote but between those who vote and the nonparticipant half of the electorate who do not vote. In effect, U.S. workers have no party to express their class interests. "What is exceptional here," the authors state, "is not the consciousness of U.S. workers but the structure of the U.S. political party system" (p. 116).

In the concluding chapter it is argued that U.S. exceptionalism can best be explained by the overriding political power of U.S. capital. The argument is unconvincing. Ironically, the authors make the same error—but in reverse fashion—as the theories they criticize. Since they found U.S. workers to be class-conscious, they therefore assume, albeit implicitly, that what must explain the lack of political assertiveness of the working class is the formidable political dominance of the opposing class. This line of reasoning is faulty since class consciousness does not necessarily provide either motivation or direction for political action. This is especially true in regard to U.S. workers. We know from numerous surveys that U.S. citizens who identify themselves strongly as members of the working class can also, with equal conviction, embrace ideas that are surprisingly conservative. Some workers have had no difficulty, for example, in supporting Jesse Jackson, having voted in previous years for George Wallace and Ronald Reagan.

It is not the dominance of capital that prevents the working class from expressing its class consciousness. This view mistakenly presupposes that most working-class people have rational and coherent political ideas to express. It is not the political power of capital that perpetuates confusion and quiescence among workers. Rather the source of the difficulty is the liberal democratic state, which is instrumental in sustaining a political system that effectively discourages workers from developing their capacity to interpret politics in terms of the reality they experience in the workplace. It is this point that Vanneman and Cannon allude to in their book but, unfortunately, do not emphasize and develop.

The American Perception of Class is a provocative and preceptive analysis of the U.S. working class. It makes an important contribution to the continuing debate on U.S. exceptionalism.

PETER BACHRACH

Temple University

Congress and Democracy. David J. Vogler and Sidney R. Waldman. Washington: Congressional Quarterly, 1985. 176p. $10.95 paper.

The authors are so demonstrably experts on the structures and procedures of Congress that a reader of this volume can rely on their accuracy and relevance in these matters and can profitably use the time and effort saved thereby to contemplate the authors' principal purpose in writing the book, which is to link congressional behavior and democratic theory and to evaluate the analytical models that they use for this purpose, namely, adversary and unitary democracy. Both of these models, say the authors, are relevant to the democratic legitimacy of Congress and should, in addition to providing a fuller understanding of the body, help citizens to make the judgments concerning institutions that democracy requires. Congress contains both adversary and unitary elements, and the book is an effort to stimulate consideration of both aspects of Congress "and thus to generate fresh critical thinking about Congress and democracy."

The adversary democracy model is the familiar interest group model—governmental decision making by partisan mutual adjustment. For Congress, it describes a bargaining process carried on by representatives of district interests, and resulting in compromises—mutual adjustments—that satisfy majorities of these representatives. The unitary democracy model emphasizes deliberation and debate in the legislative process. Whereas partisan interests that combine to form national agreements are not changed substantially by the adversarial process, the political preferences of individuals and groups that deliberate and build consensus in the unitary process are developed, shaped, and defined importantly by the process itself.

The adversary and unitary democracy models have been used by other scholars to seek a better understanding of Congress as a democratic institution. While the authors cannot be given credit for an original contribution here, their efforts to identify both the adversary and the unitary consequences of relying on particular legislative organizations and procedures are important for the study of U.S. politics. They examine from the dual perspective of adversary and unitary democracy the nature of legislative representation, of executive-legislative relations, and of political parties. They focus on both the adversary and unitary consequences of the organization of congressional committees and the use of congressional procedures. They also examine from this dual perspective the search for consensus in foreign policy and the effects of adversary pressures on budgetary policy.

The challenge today for students who are sympathetic to the authors' approach to the study of Congress (as I am) is to develop analytical rules that will describe and predict the adversary and unitary consequences of alternative institutional arrangements and alternative legislative procedures, for it is this that will ultimately provide a fuller understanding of the body and enable citizens to make informed judgments concerning their institutions. The authors of this study have only begun to make progress on this tough task.

In the context of a recent flood of studies that use rational choice models to study congressional behavior, it is refreshing to read a monograph that focuses on the relations of congressional behavior to the requirements of a democratic system of government, rather than assuming, as do many of the rational choice studies, that these relations are given or simply ignore them.

ARTHUR MAASS

Harvard University

Congress and Economic Policymaking. By Darrell M. West. Pittsburgh: University of Pittsburgh Press, 1987. 203p. $24.95 cloth, $12.95 paper.

This book is centered around Ronald Reagan's 1981 success in winning enactment of tax reduction and a shift in federal budget priorities and the subsequent stalemate between the president and Congress on budget policy. West seeks to explain Reagan's early victories in terms of the role of activists as intermediaries between citizens and leaders and his later difficulties in terms of the president's failure to seek a policy mandate in his 1984 reelection campaign.

These explanations lead West to give short shrift to more obvious explanations of Reagan's successes and failures. There is hardly a word about the Republican capture of the Senate in 1981 and of their extraordinary use of such new budget tools as reconciliation. Nor does the author say much about Reagan's adroit manipulation of public opinion or skillful private persuasion. Instead, he argues that Reagan prevailed because a tiny number of activists in key congressional districts got on the phone or sent telegrams in support of the president's legislative demands. It is not until West weaves a rather simplistic activist theory that he notifies readers that on key House votes Republican unity ranged between 98.9% and 100% (p. 72). It is highly likely that Republican cohesion in both chambers contributed more to Reagan's success than did the hypercharged letter writers and phone callers.

The author's "mandate" theory runs afoul of his own data. West insists that part of Reagan's success in 1981 "hinged on the idea that many legislators, journalists, and activists believed his proclaimed mandate and acted as if Reagan truly had a mandate" (p. 31). West's survey of House members shows, however, that barely 30% of all members voted as they did because of Reagan's mandate and that fewer than 40% of the "boll weevils" (conservative Democrats) were influenced by the mandate (p. 41). West adds to the confusion, noting that the weak link between district pressures and voting patterns casts "doubt on Reagan's claim to have succeeded in Congress because he had a classic mandate" (p. 51).

If the mandate theory doesn't really explain Reagan's 1981 success, West nevertheless trots it out to explain his failures after the 1984 election. Thus "because of the nonspecific way in which Reagan conducted his 1984 campaign, there was no way that he could claim even a perceived mandate for particular deficit reduction steps or tax reform approaches" (p. 31). Again, there is a failure by the author to take account of simpler, more compelling explanations, such as erosion of Republican membership in the House, growing Democratic cohesion, shifts in public opinion, and congressional skill in confronting the president and paralyzing the budget process.

West provides some solid data on changing voting patterns in Congress during the Reagan years, showing which bloc of members switched between the early pro-Reagan votes and the later anti-Reagan ones. Not surprisingly, he identifies boll weevils as the critical swing group, but he has difficulty pinning down why they behaved as they did. At one point he finds more of "a perceptual than an ideological basis for their votes" (p. 77); but a few pages later he states that this group resolved the cross-pressures facing it "in favor of personal ideology" (p. 82).

West's insistence that Reagan was stymied in his second term leads him to underrate seriously the president's influence on the tax reform enacted in 1986. He characterizes Reagan's proposal as probusiness, despite its shift of $120 billion in tax burdens (over a five-year period) from individuals to corporations. A fair assessment of tax reform would show that Reagan prevailed on his key objectives: a steep drop in marginal rates and revenue-neutral legislation.

In sum, this is a little book that seeks big explanation for some of the most fully covered political events of our times. West offers little that is new or revealing, but it is not for lack of effort.

ALLEN SCHICK

University of Maryland, College Park

The Guarded Gate: The Reality of American Refugee Policy. Norman L. and Naomi Flink Zucker. New York: Harcourt Brace Jovanovich, 1987. 342p. $22.95.

In a troubled area of national policy the Zuckers have attempted to draw together a current portrait of U.S. policy and practice in admission decisions for refugees, the persecuted and oppressed from abroad.

Political scientists have found little of interest in refugees and seekers of asylum in recent decades. The Zuckers see their effort as prefatory. Nevertheless, the result has several commendable qualities: *The Guarded Gate* describes in detail the agencies with an interest in determining those who qualify for admission, the services they receive and coordination with state and local governments. Conflict and machinations between the departments of State, Health and Human Services, and Justice and, not infrequently, the White House are endemic to refugee policy administration. The authors find much to regret in the frictions of interagency conflict, suggesting that the humanitarian objectives tend to be lost sight of in the pressures of larger foreign policy needs or perceived domestic political considerations. While not original, the Zuckers offer an alternative administrative scheme that, based in the White House, would divorce refugee policy from those formulating foreign policy and those charged with enforcing immigration laws.

An additional virtue of *The Guarded Gate* is a brief but balanced description of current legislative proposals, the church-based Sanctuary movement, and resettlement questions.

The Zuckers make no effort to explore or analyze the sources of antirefugee or proadmission political pressure. Their sole foray outside official Washington is the Sanctuary movement. Further scholarly attention will surely complement our understanding of the role of the public in shaping official refugee policy and implementation.

Repeatedly the authors characterize antiadmission opinion in the public as racist or xenophobic. If in some situations the characterization is accurate, it is clearly not invariably or even commonly so. Indignation pervades these pages to a distracting degree: they dismiss any concern with costs of refugee assistance as at best short-sighted. Competition for money to sustain services in language training, health, job training and income support seems as natural as it is inevitable, particularly so in an epoch of budgetary stringency. As domestic competition for these resources becomes more intense, the size of the world refugee population increases to record peacetime levels. The authors acknowledge the United States cannot resolve the problem acting alone. Domestic bureaucratic reform may lessen waste and inefficiencies. A reduction in the level of violence that generates refugees in southern Africa, Southeast Asia and Central America is the task of foreign policy. Such a welcome achievement would allow a measure of confidence that a successful refugee policy can be put into place.

In the interim, human rights and humanitarian critiques of current refugee policy are timely as a means of articulating an attainable alternative. The Zuckers identify unseemly contradictions in the admission of Argentineans as against Chileans, Nicaraguans as against Salvadorians. These contradictions, compounded by official silence, are notable in both Democratic and Republican administrations.

An informed and concerned public the authors believe can require explanation and justification. *The Guarded Gate* is a welcome step to filling that need.

JOHN CROW

University of Arizona

COMPARATIVE POLITICS

The Power of Ideology: The Quest for Technological Autonomy in Argentina and Brazil. By Emanuel Adler. Berkeley: University of California Press, 1987. 398p. $45.00.

Adler has combined a set of fascinating case studies with a bold theoretical statement of how social change occurs to produce an important and provocative book. It is well researched and written with verve and conviction and will be an essential point of reference for anyone studying third world efforts to participate in high-technology industries.

A general review of 20 years of science and technology policy in Argentina and Brazil is combined with detailed case studies of the contrasting strategies of Brazil and Argentina in developing indigenous nuclear and computer technologies to form the empirical foundation of the analysis. The four case studies make an elegant set. There is a success and failure in each industry and each country. Brazil succeeds in computers but fails in nuclear energy; Argentina does the reverse. Adler argues that no structural difference between the two countries or the two industries accounts for this pattern of success and failure. Argentina had no material advantage with respect to the production of nuclear fuels nor any disadvantage with respect to computer technology, nor was there any converse set of advantages and disadvantages in the Brazilian case. The absence of structural differences suggests that ideological and institutional factors must account for the differences, and this is the point of the book.

Adler's aim is to convince the reader that independently of (and sometimes in opposition to) structural factors, groups and individuals may develop collective understandings that allow them to become "weathermakers" (p. 88), technocrats who can "catalytically affect threads of events" (p. 327), producing structural outcomes that would not otherwise occur. The case studies illustrate his contention nicely. Jorge Sabato and his colleagues in the Argentina Atomic Energy Commission (CNEA) constructed a vision of technological development, institutionalized it in the CNEA, sold it to political and military elites, and succeeded in making Argentina one of the few third-world countries to dominate the nuclear fuel cycle. The "pragmatic anti-dependency guerillas" of Brazil's CAPRE Commission for the Coordination of Electronic Data Processing managed in similar fashion to engender an indigenous computer industry in Brazil.

By the end of the book the reader will have enjoyed a set of well-crafted stories and been convinced of the overall point but is also likely to feel a lack of closure. What is lacking is a systematic comparative conclusion assessing why "pragmatic anti-dependency guerillas" were successful in one case but absent or ineffectual in another. The three pages of text that comprise the concluding chapter reiterates the initial theoretical assertions rather than trying to gain analytical leverage from the comparison of the cases. Why was the CNEA in Argentina able to enjoy "continuity of leadership and political autonomy, consensus and insulation" (p. 280) while Brazilian nuclear technology could construct no comparable institution? Why were those attempting to build computers in Argentina overcome by "fracasomania" while those in Brazil were not? It is all very well to suggest to other developing countries that embodying optimistic, forward-looking ideologies in insulated institutions will facilitate technological development. The question remains, "What circumstances and what strategies make ideology an effective weapon?" We are given two interesting positive instances but deprived of an analytical model that would allow us to predict outcomes in other industries and countries. Having convinced us that ideology is important, Adler will hopefully go on in his future work to provide us with a more thoroughly worked out schema of when and how the ideologist may become a "weathermaker."

PETER B. EVANS

University of New Mexico

Electoral Patterns and Perspectives in Mexico. Edited by Arturo Alvarado. San Diego: University of California, 1987. 287p. $15.00 paper.

Things Mexican are increasingly significant in the United States. Mexican petroleum powers our automobiles, Mexican antiquities decorate our mantels, and tacos satisfy our palates. For the last dozen years or so Mexican political stability has also captured the attention of U.S. policymakers and, by extension, the academic community.

Flowing from that general focus, the Mexican electoral process has elicited growing attention. Beginning with the elections of 1985, U.S. policymakers and journalists have given a fair amount of time and effort to describing and analyzing the Mexican electoral system and the country's elections. The academic literature is still in its infancy, but some beginnings are underway. Arturo Alvarado's collection makes a nice contribution to that incipient literature.

The study looks most specifically to the elections of the summer of 1985 at both the federal and state level, but it also includes several more broadly gauged studies concerned, variously, with political attitudes along the border, the history of political movements in several states and *municipios*, and the significance of the U.S. press to the Mexican electoral system. The book counts 12 chapters, 10 written by Mexicans, mostly from the national university and from Tijuana's *El Colegio* de la Frontera Norte. Two U.S. authors also contribute chapters.

The chapters are uneven, but all are adequate, most are good, and a few are particularly noteworthy. Enrique Márquez's chapter on the history and political significance of *Navismo* in the state of San Luís Potosí is certainly first-rate. Salvador Nava Martínez' rise to the mayor's position *(presidente municipal)* in the city of San Luís Potosí documents much of successful minority party political strategy in Mexico. The analysis is also germane to an understanding of state-municipal relations in Mexico. In the contemporary context, the official party's candidate in the 1988 presidential elections, Carlos Salinas de Gortari, promises a "new politics" respecting the rights of the minority candidates to win more state and local positions. Márquez's analysis may be quite timely.

Jeffrey W. Rubin's case study of the COCEI in Juchitán, Oaxaca is equally valuable in documenting and analyzing a challenge from the Left in Mexico. Perhaps more important for U.S. journalists and policymakers, it balances the exaggerated attention that they have given the conservative opposition in the form of the Partido de Accion Nacional (PAN) in northern Mexico.

Drawing from a survey in the North and the Federal districts, Alberto Hernández Hernández contributes important documentation in "Political Attitudes among Border Youth." They tend to be more *PANista* than their cohorts in the *Distrito Federal*; more significantly, they tend to be more *PANista* than their elders. Graciela Guadarrama S. compares and contrasts the political attitudes and activities of businessmen in the states of Sonora and Nuevo León. In the process, she sets out a splendid typology of the gradations of political activism among her respondents. Rocio Guadarrama offers a rather tedious discussion, "Elections in Sonora," but it is chockful of excellent data and sound analysis.

Several significant points embellish the book in more than one of the chapters. U.S. journalists' and policymakers' preoccupation with Mexican electoral fraud and putative violence ill serves them in their attempt to fathom the deeper meaning of electoral politics within the Mexican system. PAN is certainly increasing its electoral support, but it is still far from possessing the ability or the strength to pretend to be an alternative to the official party on the national level. The so-called *Neopanistas* imply stresses and strains within the party. By comparing electoral returns over time or by comparing them with survey research data, analysis of electoral returns can be a fruitful research exercise even in Mexico's authoritarian system, frequently characterized by electoral fraud corrupting the validity of the official returns.

In a more negative vein, the thematic coherence slips off the mark in several of the chapters; they are only broadly and indirectly on the pronounced target of the book. As implied, several of the chapters are not well done. The book includes neither index nor bibliography. A bibliography would have been particularly useful, given the scarcity of literature.

Overall, Alvarado's volume makes a nice contribution to an area crying out for careful and imaginative research. It is recommended for Mexicanists and students of comparative parties and elections.

EDWARD J. WILLIAMS

University of Arizona

Economic Development: The History of an Idea. By H. W. Arndt. Chicago: University of Chicago Press, 1987. 226p. $20.95.

Pioneers in Development: Second Series. Edited by Gerald M. Meier. New York: Oxford University for the World Bank, 1987. 244p. $29.95.

Each of these books is an exercise in intellectual history. Arndt's is a comprehensive review of economic "development" as an idea and therefore traces how various seminal writers have approached this concept as an end towards which all societies have ostensibly striven. Meier's edited volume, the second in a World Bank series, Pioneers in Development, is a collection of essays by many of the very scholars about whom Arndt writes. Meier's task is also more limited, namely, to examine the return to primacy among developmental economists of neoclassical economics as both an analytic tool and a vehicle to provide policy recommendations. As political scientists might anticipate, however, neither volume pursues the political dilemmas likely to be faced by developing country elites if they were to adopt the recommendations offered by the bulk of developmental economists.

Arndt deals with the changing meanings of development over the last century and is less concerned with means proposed to achieve this end. While acknowledging that development economics did not achieve the status of an academic discipline until World War II, he rightfully begins his analysis with a lengthy discussion of the prehistory of development thought. In this chapter, Arndt surveys such luminaries as Smith, Ricardo, and Marx. Moreover, he judiciously summarizes the developmental goals of Meiji Japan, China under Sun Yat-sen, and the colonizers of Asia and Africa before the period of independence. The reader is left with little doubt that development as an idea was important in the economics literature before it became a subfield after World War II. Indeed, the contributions of the earlier scholars laid the essential groundwork for the more recent neoclassical contributors.

The bulk of Arndt's book is devoted to a discussion of the now-familiar conceptions of development as aggregate growth, growth and redistribution, satisfaction of basic human needs, technological progress, and, ultimately, an agglomeration of most of these goals. Only in his chapter on "Development as Growth" is there found a discussion of strategies designed to achieve such ends. Broadly speaking, the means proposed were physical capital formation, human capital formation, and trade as the engine of growth.

Perhaps Arndt's most important contribution is his discussion of the "right radical counterpoint" to the development literature. Here he deals with a variety of perspectives ranging from Ghandi's initial rejection of Western modernization prescriptions to "the Ayatollah Khomeini's condemnation of capitalism and communism alike as evil, corrupt and unclean" (p. 167). Equally valuable, however, is Arndt's recognition that when faced with the exigencies and responsibilities of governance, all rightest leaders have focused to varying degrees on economic and social development. Finally, unlike many development economists, Arndt recognizes that nearly all third-world leaders ultimately have accepted development strategies that are "most likely to keep them in power: aiming, within the constraints imposed by conflicting pressures of interest groups and by underdevelopment itself, at the highest attainable rate of economic growth" (p. 173).

The weakest portion of *Economic Development* is the section on the left radical counterpoint to neoclassical development approaches. In particular, the dependency approach is given short shrift. While many of Andre Gunder Frank's hypothesis are paid homage and duly demolished, the entire school of thought labeled "dependent development" is mentioned only in passing in *one* sentence. For example, there are no references to the important works of Peter Evans.

Contributors to *Pioneers in Development* include such familiar figures as Theodore Schultz, Gottfried Haberler, Hla Myint, Arnold Harberger, and Celso Furtado—the last a structuralist who continues to offer alternative explanations and policy prescriptions to his neoclassical counterparts. Topics covered range from agricultural and trade policy to social project evaluation. Each essay is also followed by comments from other well-known economists.

Both Schultz and Haberler explicitly argue for a "monoeconomics" approach to development, refuting the notion that distinct theories are necessary for developed and less-developed countries. Moreover, as Meier notes in his excellent overview of the essays, all of the neoclassical authors agree that excessive governmental planning in the 1950s was the primary cause of low economic growth rates. For Schultz, the best path to increased agricultural productivity is a relatively unfettered market pricing system, and Haberler argues just as forcefully for third-world incorporation into the international capitalist trade system. Haberler asserts incorrectly, however, that the success of the Asian "Gang of Four" newly industrializing countries is in large measure due to a lack of governmental planning as regards their exported development. In his critique of Haberler's essay, Ronald Findlay observes that only in Hong Kong "has something close to laissez-faire been practiced. In the case of the others there is extensive intervention and promotion in the form of state enterprises, subsidies, regulations, and other measures affecting the capital market, domestic savings, the trade regime, and indeed almost every aspect of the economy" (p. 96).

Only in Furtado's essay on the structural causes of underdevelopment does one find a discussion of the "political" factors inhibiting development. Many readers will not find his arguments entirely convincing, especially his familiar class-analytic approach that credits peripheral elites with virtually no room for maneuver in the international political economy. Nonetheless, he is the most explicit of all the contributors in his recognition that "good policies" will not automatically be accepted by leaders who believe that such policies will terminate their political careers. Nyerere's accommodation of the Tanzanian middle class comes immediately to mind.

Interestingly, Meier himself is the most astute observer of the realities of choice in the third-world. His brief introduction is a masterful synthesis of the various contributions to this book, but it is also a summary of the development experience in the periphery over the last 40 years. He quite correctly states, "If development policy is to improve, the political cannot be taken as simply exogenous and beyond economic analysis" (p. 10). In this regard, the editor mentions in a footnote Bates's classic *Markets and States in Tropical Africa* (1981).

Each of these books provides a stimulating overview of the history of economic development thought. Each in its own way helps the reader understand why there has been a significant resurgence recently in the neoclassical approach to development. Whether this resurgence has been translated into practice via the policy process is still problematic. It is the discontinuity between theory and practice that will provide the more interesting areas of inquiry in the future. And it is these questions that can be answered perhaps only when economists and political scientists realize that they can no longer work in splendid academic isolation from one another.

DAVID W. LOEBSACK

Cornell College

Book Reviews: Comparative Politics

The Invisible Bureaucracy: The Unconscious in Organizational Problem Solving. By Howell S. Baum. New York: Oxford University Press, 1987. 214p. $24.95.

The purpose of this book is to demonstrate that the constraints presented by bureaucracy are created not only consciously by bureaucrats but unconsciously as well, which represents a salient and overlooked deterrent to organizational performance. Through a psychoanalytic approach, Baum investigates the inner workings and feelings of bureaucrats about their work in order to provide insight into the psychological structure of bureaucracy.

Baum's thesis is quite interesting. He suggests that the formal constraints of bureaucracy (e.g., hierarchy, centralization, etc.) are often magnified by bureaucrats' own unconscious submission to those constraints. In effect, bureaucrats unconsciously create an "invisible bureucracy" which, rather than formal or conscious bureaucracy itself, is often responsible for ineffective or poor performance. Bureaucrats become so constrained by the invisible bureaucracy that they often overlook opportunities to deviate from formal rules and regulations and to employ their own creativity and common sense in organizational problem solving.

For example, Baum points out that hierarchy makes bureaucrats unconsciously anxious about "being shamed by superiors for poor performance" (p. 5). In order to shield themselves from shame, bureaucrats may "avoid taking initiative . . . ritualistically recite rules, and even doubt their own competence to act" (p. 5). In so doing, bureaucrats divert valuable time and energy away from their job duties, or "conscious goals," to the ultimate detriment of the organization's clients and constituents.

Based on interviews with 77 bureaucrats (27 "public administrators" and 50 planners), Baum concludes that the unconscious beliefs and feelings of bureaucrats strongly influence how they perceive, approach, and respond to their work. He finds that consciously, workers seek to respond to organizational problems in a "rational" manner. Unconsciously, however, they are "more concerned about avoiding anxiety associated with personal relationships with others, even when doing so leads them away from their formal responsibilities" (p. 5).

Although the theme of the book is significant, there are some conceptual and methodological flaws in the study. Baum recognizes some of them but does little to address them. For example, the small sample size certainly raises questions about the validity and reliability of his findings. In addition, the bureaucrats were drawn only from organizations in the greater Maryland area. Although the author does get a good cross-section of local, state, and federal government bureaucrats, restricting the sample to this one geographical area leads to questions regarding the representativeness of the sample. Also, Baum never presents his findings in a concise, coherent fashion. Rather, case examples are dispersed throughout the book in a fragmented manner.

Another shortcoming lies in Baum's tendency to focus primarily on behavior that is defensive, where bureaucrats seek to avoid shame or guilt. By emphasizing such behavior, Baum perhaps makes some unconscious assumptions of his own about bureaucrats. There are, however, unconscious behaviors that are not quite so "negative." And, they, too, reflect bureaucrats' efforts to manage personal relationships. For example, formal bureaucracy tends to isolate and separate people. Unconsciously, bureaucrats may seek to compensate for the detachment by cultivating social ties. This may divert attention away from explicit goals but in the long run may have a favorable effect on bureaucratic performance.

Baum also does little to address the fact that because public organizations are extremely political, the defensive behavior to which Baum is extremely attentive is not only acceptable but "effective." Such behavior may reflect a political astuteness on the part of bureaucrats to be sensitive to their superiors *and* constituents.

This leads to another important, but unaddressed question: Who are the superiors? In other words, are the bureaucrats (or at least the public administrators) who were interviewed midlevel managers or "senior executive servants"? Are they political appointees or career servants? The position one holds no doubt affects one's feelings and beliefs about organizational power and politics. So, too, would the profession. That is, there are some fundamental differences between the professions of planning and public administration that should have been controlled for. It would seem that one's position and profession would shape the "invisible bureaucracy" that Baum blames for bureaucrats' sense of ineffectiveness or incompetence.

Finally, there is something to be said for adhering to bureaucratic rules and regulations, whether consciously *or* unconsciously, as the experts in administrative law will tell us. Making a decision that is creative or based on common sense may have serious legal and constitutional implications that could be harmful and costly not only to the individual bureaucrat but to the organization as well.

Despite some of these shortcomings, the book addresses a relatively untouched dimension of organizational study. Scholars and practitioners interested in gaining a more comprehensive picture of how bureaucracy works will find value in *The Invisible Bureaucracy.*

NORMA M. RICCUCCI

State University of New York, Albany

Social Movements and Political Power: Emerging Forms of Radicalism in the West. By Carl Boggs. Philadelphia: Temple University Press, 1986. 288p. $37.95.

Carl Boggs's *Social Movements and Political Power* adds to the growing number of books on the crisis of socialist thinking and the Western Left's search for a new post-Marxist vision. Boggs believes that ecological, feminist, peace, and countercultural movements constitute the beginning of a new progressive alternative and he expresses his "sympathies mostly with the radical side of the new movements" (p. 19). In this sense, Boggs's book can be read as a work on New Left normative political theory. Yet it is also relevant for analytical comparative politics because it attempts to prove the proposition that Socialist parties in the Mediterranean (Spain, France, and Greece), new progressive populist politics in the United States, and ecology parties in West Germany and other northern European countries share similar political ideas and constitute efforts to break the stalemate of social democratic policy in the Keynesian welfare state and to overcome an unappealing Marxisim-Leninism that built on the revolutionary potential of the working class. In Boggs's view, all three new forces are inspired by the movements of the 1970s and 1980s and, at least potentially, point towards a new strategy against conservatism.

According to Boggs, new social movements have been stimulated by economic crisis, the rigidity of corporatist interest intermediation and the bureaucratization of politics (pp. 21-38). The new movements have attempted to initiate a participatory transformation of modern society and to create a civil society with a public sphere uninhibited by state power and private corporations. They ask for social change in a multiplicity of issue areas (pp. 38-47), shun comprehensive ideologies, and seek participatory forms of organization (pp. 47-57). They criticize Marxism as an ideology and political practice that shares with its bourgeois antipodes a hierarchical, economy-centered view of society (pp. 57-74).

Eurosocialist parties in southern Europe built on the participatory vision of the new movements. Yet they failed to realize these ideals when they took office and had to cope with the economic problems of their countries (chap. 3). U.S. neopopulist political forces, for instance the Campaign for Economic Democracy, also endorse the movements' broad objectives but confine themselves to limited, pragmatic reforms once they run municipalities, as they have in Santa Monica, Santa Cruz, and Burlington, Vermont (chap. 4). Therefore, Boggs pins his hopes on the West German Greens as the agents of a post-Marxist radical synthesis, linking social movement protest with the electoral struggle for government power and pursuing a participatory, egalitarian, ecological, and pacifist transformation of modern society (chap. 5). Just as Eurosocialists and U.S. neopopulists, however, the Greens still lack a transition strategy and underestimate the exigencies of political organization (chap. 6).

While presenting a stimulating, broad comparative idea and a wide range of valuable observations, Boggs's book provides little evidence to justify his main argument that southern Eurosocialism, U.S. populism, and northern European ecologism serve as functionally equivalent responses to similar problems and movements. Mediterranean Socialist parties have never had more than a very tenuous linkage to new social movements. To the contrary, the parties' electoral strength and policy may have benefited from the weakness of social protest movements that would have forced them to pursue more radical social change. The U.S. neopopulists tend to be more concerned with some of the basic needs of equality and economic security that are already addressed by northern Europe's comprehensive welfare states than the themes that drive European social movements and ecology parties. Thus there appears to be little similarity between the three political forces Boggs has tied together other than that they are equally distant from conventional social democratic Keynesianism. Perhaps Boggs glosses over these fundamental differences, for his book does not present a sufficiently detailed, precise comparative framework and explanation why regions of the advanced capitalist democracies have developed distinct varieties of leftist politics.

Overall, Boggs's book is a useful barometer of New Left thinking. Moreover, it is well written, clearly organized, and focused on its main propositions. Even if one disagrees with its argument, the book's stimulating ideas may help us to search for a better understanding of new political forces in Western democracies.

HERBERT KITSCHELT

Duke University

Prospects for Change in Socialist Systems: Challenges and Responses. Edited by Charles J. Bukowski and Mark A. Cichock. New York: Praeger, 1987. 151p. $34.95.

This book claims to "focus on the issue of change in a systemic sense" among the "Socialist" states (Socialist by Soviet definition). The editors feel that change is necessary in the socialist system in the 1980s because of, among other things, the aging (and occasional disability and death) of the communist hierarchs. The contributors concentrate their commentaries on recent history and present conditions and speculate as to how particular political systems might respond to the challenge of change.

Part 1 is entitled "Social and Economic Change." Richard Farkas first discusses the general notion of change in Soviet-type systems; whether it is nonexistent, glacial in movement, or quick enough for timely reform. He then turns his attention to whether these systems can deal with "performance based management of society" (p. 6). Can these regimes stabilize and obtain legitimacy, or will the Party view "developmental engineering" as intruding on the Party's self-proclaimed right and duty to rule? Author Farkas advises would-be reformers to be "patient, persistent and polite" (p. 16)—a formula taken from another context—and to work within the system, since there is no other way. Paul Maren's "Economic Policies and Options for Eastern Europe" deals with the six bloc countries plus Yugoslavia (but not Albania) in regard to Western loans, balances of payment, noncompetitive goods, and trade deficits of Centrally Planned Economies. His discussion is buttressed with several readable tables, but it is rather unenlightening in content and pedestrian in conclusion.

Part 2 contains four specific country case studies: Czechoslovakia (chap. 3), Hungary (chap. 4), Poland (chap. 5) and Yugoslavia (chap. 6). Chapter 7, Daniel S. Papp's "The Soviet Union and Non-European Socialist States," should really have become part 3, since it is neither a country study nor an analysis of social and economic change but rather an international relations discussion. It deals with Soviet definitions of such "Socialist path states" as Afghanistan, Tanzania, and 13 others, as opposed to "Socialist states" of which there are five outside East Europe—China, Vietnam, Laos, Kampuchea, and Cuba. Papp details how the Soviet union deals with the Indo-China states, Cuba, and China. He predicts cautious improvement of relations with China, the retention of Cuba as a base in the Western hemisphere, and an expansion of Soviet presence in Indo-China. He has produced a rather conservative argument strongly supported from the literature. This is the best chapter in the book, though out of place.

Returning to the country studies, Sharon L. Wolchik looks at Czechoslovakia in regard to economic performance and political change. She feels economic performance has not been all that good and that stagnation without hardship will continue. The regime, without real popular support, will muddle along following the Soviet lead.

Hungary, while proclaiming a more extensive socialist democracy than Czechoslovakia and championing "enterprise democracy," will not be able to do much better than Czechoslovakia. Barnabas A. Racz looks at enterprise democracy and electoral reform and concludes, "Socialist democracy reforms could remain meaningless for lack of favorable public response" or else could "contribute to stability and strength of the Socialist system through expanded participation" (p. 75).

Maurice Simon, a well-known observer of Poland, summarizes the 1956-85 period and concludes that the present communist regime is based on the military and rules by selected coercion. He then discusses youth attitudes toward the regime based on a survey of 650 Warsaw University and Warsaw Politechnic students. The results were negative about nearly everything concerning the regime but not towards socialism as an idea—a conclusion most scholars would reach without such a survey. Simon's rather unsurprising conclusion is that the communist Polish United Workers party still confronts a legitimacy crisis and will continue to do so.

The editors contributed jointly to a chapter on Yugoslavia. They outline the decline of Yugoslav Gastarbeiter program in West Europe and the concommitant decline in hard currency remittances. Yugoslavia also suffers from a North-South crisis, with heavy unemployment in the south. This, combined with a troubled economy, foreign debt, and a falling standard of living has created real problems. The authors, however, feel that Yugoslavia can cope with these crises if it recognizes them and makes moves to confront them.

What is the audience for this work? This book could not be used in an undergraduate course because too much knowledge is presupposed. Graduate students will find that the conclusions are in no way new. In fact, the editors do not even reach an overall conclusion. I cannot recommend this book except perhaps as a reference work. The bibliography is good and some of the footnotes could be valuable to a researcher.

PAUL J. BEST

Southern Connecticut State University

Great Expectations and Mixed Performance: The Implementation of Higher Education Reforms in Europe. By Ladislav Cerych and Paul Sabatier. Trentham, UK. Trentham Books, 1986. 276p. $13.95 paper.

This book is an analysis of case studies about reform of higher education in Europe during the 1960s and 1970s, using British, Swedish, Polish, Norwegian, French, and German examples. Its analytic approach is to construct case studies around a common set of issues and then use a conceptual framework to analyze cross-cutting themes. The framework for this study will be familiar to students of policy analysis and policy implementation, since it is an adaptation of Sabatier's earlier work with Daniel Mazmanian, *Implementation and Public Policy,* (1983).

The range of higher education reforms covered by the analysis is broad and interesting. It includes some familiar examples: the British open university; the Norwegian and Swedish experiments with creating regional institutions and increasing access to universities. It also includes some less familiar examples: the Polish preferential point system to increase access to higher education for students from worker and peasant families and the French attempt to establish a new tier of two-year technological institutes. The cases give a useful overview of selected attempts to broaden access to and change the institutional structure of higher education.

The authors' main analytic argument is that the reforms failed to achieve their major stated goals even while they succeeded in modest ways. The reasons for their limited success, the authors argue, can be traced to a failure to meet the conditions of successful implementation that apply to any policy. This argument will not come as a surprise to those familiar with Sabatier's earlier work. In essence, this work sets up a list of conditions for successful implementation (goal clarity, sound theory, support from key implementation agencies and interest groups, etc.) and proceeds to demonstrate policies' congruence—or lack of congruence—with these conditions. Lack of congruence, the argument says, leads to failure, where *failure* means failure to achieve initially specified purposes.

It should also come as no surprise to students of liberal reforms of the 1960s that these specific reforms of higher education were often not carefully thought out on key dimensions and often embodied no particularly clear or consistent set of purposes or underlying causal model. For an analysis to demonstrate these things is interesting but hardly earthshaking.

The main weakness of the framework underlying the authors' analysis, then, is that it subjects policies to a rationalist—some would say rationalistic—set of criteria that, for the most part, we already know are not embodied in policymaking. This tension between a rationalist model of policy analysis and a messier, less determinant institutional and political reality allows the authors to draw conclusions about obvious defects of policies without subjecting their own model to a test of descriptive validity. Hence, the analysis has less utility for understanding the limits of a particular form of rational analysis than it does for understanding the gaps between rational analysis and political behavior. We already know much more about the latter than the former.

On balance, then, this book will be a useful addition to the libraries of scholars with an interest in higher education policy and a somewhat less useful but still significant addition to the literature on public policy.

RICHARD F. ELMORE

Michigan State University

Party Systems in Denmark, Austria, Switzerland, the Netherlands and Belgium. Edited by Hans Daalder. New York: St. Martin's Press, 1987. 372p. $35.00.

In the foreword Professor Daalder, editor of this work, explains its raison d'etre: "This book presents the development of the party system of five smaller European democracies since World War II. . . . The major aim of the research project is to offer a critical analysis of widespread but untested generalizations about 'waning' or 'crisis' of European party systems. Parties, so a standard argument goes, have been losing in influence in comparison to other political actors." The key purpose of this collection of party studies is to assess whether parties have conformed to the reports of their demise. To engage in the analysis Daalder has chosen a set of countries off the beaten academic track (Denmark, Austria, Switzerland, the Netherlands, and Belgium), a group of respected analysts from each of the countries, and a framework to which he asked each to subscribe. In keeping with modern times the outline is minimalist to accommodate varying priorities of interest among the writers. Daalder states, "At the core of that outline was the assumption that changes in parties and party systems could originate at any of three levels in the political system: in the interaction of parties at the level of government and parliament; in the relation between parties and voters; and in the internal development of parties. A further important concern was a discussion of the relative 'hold' of parties on the political process, in comparison with other possible actors [e.g. interest groups]. Finally, authors were asked to indicate possible reform movements in either the institutional or party-political arena" (p. xii).

Each of the writers has a solid research record and a reputation within political science that, with the exception of Daalder's, is known better in Europe than in the United States. Each has also taken on the task of writing an academic treatise in a foreign language, (English), something few U.S. political scientists would venture.

In general the volume succeeds in what it sets out to do. Each of the authors uses the basic outline prescribed by Daalder and modifies it to suit his particular country. In every case there is a great deal of data made available so that the reader can judge the validity of interpretations put forth. Seventy-six tables appear in the book and include descriptive as well as analytic statistics. The analysis, however, is plain and intelligible to anyone with an interest in political science. Writing styles vary from the terse approach by Austrian Peter Gerlich to the almost chatty language of the Swiss Henry Kerr. The length of the chapters also varies greatly. Although the English is always correct, at times an appreciation or perhaps a toleration for Germanic paragraph and sentence constructions is in order to get the full meaning of an author's point. This slight inconvenience should not deter readers from pursuing the insights offered.

The analysts agree that their countries' party systems are not in serious danger. The single generalization that emerges is that in spite of changes in social structure, the emergence of single-issue groups and parties, media candidate marketing, and new cross-class concerns about the environment, parties have adapted to these new realities of the large-scale political system and used them advantageously, if not always optimally. In Switzerland policy still requires a consensus of groups, parties, and the bureaucracy to be successful. In Belgium the linguistic and religious communes as now represented by party wings are still part of the consultative process in policy formation. Yes, there is more personalization of politics, just as there are more fast food chains and hypermarkets in Europe as a whole than there were 20 years ago. To form governments, however, parties are still necessary, and if they are necessary for this primary ruling function, their position in the larger system cannot be denied even with the institution of some of the party-gutting reforms and trends that have become apparent in the last two decades. The writers indeed document that parties have changed in structure and approach to the electoral arena, but they are still parties by any but the most obscure definition. *Plus ça change, plus c'est la meme chose.*

The greatest merit of the book is the exposure it gives to the smaller democracies of Europe. Britain, France, Germany, and, to some extent, Italy and Spain seem to get an inordinate amount of attention in research and in the classroom. With this work new data on different countries can be introduced to break the parochialism of intro-comparative class lectures. For the reader, there emerge new and useful opportunities to try conceptual frameworks in other national contexts. For example, since all of the countries treated use a form of proportional representation voting system and have significant nonclass communal differences, one can explore variations on these themes as they are reflected in party politics.

The serious, research nature of the volume is affirmed by the care the authors take to give their work broader relevance. Each tries to place his study in the context of the field's literature and the other works in the Europe Party System series. All of the cases have significant reference sections of general works in the area of parties, most of which are in English. Each work also has a country-specific set of references written mostly in the local tongue. Reference to traditional as well as newer works in comparative politics are made with the presumption of acquaintance, putting the book at the graduate seminar or the undergraduate tutorial level. For researchers in comparative parties, it provides a compact, ready reference to the unique and more common aspects of these less-treated systems through the comprehensive general index. Work hitherto only in local language is brought into the mainstream of research in comparative politics. This competently written and presented book belongs in libraries in the political science collection and on the shelves of individuals with an interest either in comparative politics or in political parties in general.

CONRAD RAABE

Loyola University, New Orleans

Human Rights in Contemporary China. By R. Randle Edwards, Louis Henkin, and Andrew J. Nathan. New York: Columbia University Press, 1986. 193p. $25.00.

The book consists of four essays concerned with individual rights and state power in Chinese thought and practice, one essay each contributed by Edwards and Henkin and two by Nathan. They resulted from a series of discussions held by the authors over a period of three years. To varying degrees, the contributions attempt to combine scholarly analysis with advocacy of human rights, not always to the benefit of the former. The authors disagree about the potential of Chinese political and legal tradition to foster or allow an increased respect for individual rights in the People's Republic of China (PRC).

Henkin's essay "The Human Rights Idea in Contemporary China" is written primarily from the standpoint of an advocate rather than that of an analyst—and a starry-eyed advocate at that. He defines human rights as "claims which, it is agreed, every individual has, or should have, upon the society in which he/she lives" by virtue of that individual's humanity (p. 8). As to the content of these claims, they include all of the political and civil rights and "economic, social, and cultural" rights enumerated in the Universal Declaration of Human Rights and later covenants. All governments should be able to comply with these documents because they were "couched in terms that would not render them unacceptable or uncongenial to any particular ideology or society" (pp. 10–11). Although Henkin devotes much space to celebrating the tradition of limited government and individual rights as found in the United States and noting the absence of such a tradition in China, he concludes that "socialism and the Chinese system" are just as compatible with the content of the human rights documents as "free enterprise and U.S. democracy" (p. 29). There has been insufficient effort to persuade authorities in the PRC to respect human rights (p. 34).

Nathan, on the other hand, sees the Chinese legal and political tradition as a major obstacle to progress in the area of individual rights. His essays, which are the best researched of the contributions, seek to identify a constitutional tradition in China and illuminate the intellectual and sociopolitical conditions that produced it. He examines the treatment of political rights in 11 constitutional texts from four regimes in China: the last imperial dynasty, the pre-Nationalist republic, the Nationalist republic, and the PRC. The central idea he discerns throughout is the harmonious rather than adversarial relationship between the state and the citizen. In addition, individual rights are privileges granted by the state, to be delivered as it deems fit. Constitutions are programmatic, enumerating various rights as goals of the state, not as claims against it. Nathan finds this tradition emerging from a confrontation between traditional ideas, modern national needs, and imported political and legal concepts. Underlying the constitutions is a philosophy of law as the will of the state, a concept that he notes has had few competitors in Chinese history and was reinforced by borrowed foreign philosophies. In the modern era, China was a weak state invaded by strong democracies. Chinese legal scholars saw constitutions as a basis for the strong state in Europe and later in Japan. German concepts of law and the state that denied ideas of "natural rights" came to China in the nineteenth century by way of the Japanese, who themselves viewed constitutional government as a solution to the problem of state weakness. At the same time, the Confucian tradition contained no idea of legitimate individual interest to counterbalance the idea of the strong state. In the Confucian worldview, "private" interests are group interests—family, lineage, community—and the great man is he who serves society. Despotism is bad, but it is bad because it crushes the impulses of the literati to contribute their creativity, and the masses their energy, to the realization of common goals shared by state and society. Thus the fundamental goal of all the framers of Chinese constitutions throughout the past century has been to strengthen the state and promote the collective welfare rather than protect the individual against excessive state power.

Like Henkin, Nathan chooses to contrast China with the United States. He does so to highlight the gap between the two traditions with respect to the importance accorded the state versus the individual and thus call attention to rights abuses in the PRC. He presents the contrast as one between liberal-democratic and socialist states (pp. 79-82). This is misleading because his entire focus is on the continuity in legal and political thought among the various regimes in China in the last century. He does not analyze China as a "socialist" (or, rather, Marxist-Leninist") state as such; Marxism-Leninism is dealt with by Nathan as an imported ideology that reinforced the long tradition of Chinese thinking about state and society. Aspects of Marxism that conflict with the tradition (notably, the concept of the state as an instrument of class warfare) are not stressed.

In contrast with Henkin and Nathan, Edwards's essay minimizes the contrast between Chinese and Western traditions with respect to the state and individual. He describes numerous ways in which civil rights are still seriously limited in China (as of 1982) but argues that the PRC may be evolving in the direction indicated by international human rights documents. Such an evolution is possible because Chinese thought has a "deeply held belief in an innate human right to life and to a just result in a legal proceeding," ideas that have "constantly reemerged in Chinese substantive law and judicial process." Moreover, "the scope of expectations (about rights of all kinds) to which the average Chinese citizen feels entitled has expanded in an evolutionary fashion with the increasing complexity and prosperity of society" (pp. 74-75). Unfortunately, nothing is offered to support either of these assertions, which deserve more attention. If developed, they would have provided a more interesting counterpoint to Nathan's arguments and might have led to a discussion of what factors *do* contribute to the emergence of democratic values under various conditions, other than pressure from well-meaning outsiders.

The authors state that they hope the book will be of interest to "those interested in the state of rights in China" (p. 6), whom they assume to be human rights specialists and activists and China specialists. The effort to place Chinese legal and political thought in a comparative context should make the book of interest to others as well. However, the quality of analysis in the essays varies considerably and the treatment of the U.S. and Chinese legal and political traditions tends to emphasize the contrasts in a way that (especially in the Henkin essay) belabors the obvious.

CONNIE SQUIRES MEANEY

University of California, Berkeley

Small Nations, Giant Firms. Louis W. Goodman. New York: Holmes & Meier, 1987. 181p. $39.50 cloth, $19.75 paper.

Louis Goodman takes as axiomatic that developing countries cannot pursue self-reliant development strategies but are inevitably bound to a web of transnational financial, trade, and investment relations that they need to manage if they are to survive and develop. Further, he rightly argues that there will inevitably be a conflict here—the one he is concerned about is between the interests of these countries and transnational corporate investors—and that this conflict will be even greater for the very small developing nations. A core argument in this book is that such conflicts can often be managed in ways that benefit both parties if small nations and giant firms better understand how investment decisions are made.

The heart of the research is a set of in-depth interviews with managers of six transnational corporations (TNCs) in home offices and their subsidiaries in the large nation of Brazil and the small nations of the Andean Common Market. He also interviewed host country government officials. Goodman skillfully draws from the wide literature on TNCs and develops eight hypotheses aimed at understanding TNC investment decisions, testing the influence of such factors as the host country's market structure, the cost and availability of inputs, local government attitudes towards TNCs, and constraints on the executive's freedom to manage. This detailed work on how decision processes are affected by business environments and by organizational factors within the TNCs is bound to be of much interest both to TNC executives and host country government officials.

In important theoretical discussions (in chaps. 4 and 6), he challenges neoclassical theories of decision making derived from microeconomic theory of the firm treating firms as abstract units that make decisions on the optimal use of inputs and outputs in order to maximize profits. He argues that for marginal decisions like investments in small countries (decisions that nevertheless have an important long-term impact on both the firms and host countries) there are other criteria and objectives.

This theoretical discussion is enriched by what is one of Goodman's most interesting findings: in TNC decision making in small nations the key factor is not so much profit maximization (as it is in countries like Brazil) but the "efficient use of executive time." In the small country markets of the Andean region, TNC executives were particularly concerned with restraints on their freedom to manage. If a project looked like it would be complicated, troublesome, and time-consuming to manage (for them or their home office bosses), they would likely steer clear of it.

This finding is particularly important because the relationships between TNCs and developing countries has become increasingly complex in recent years as both parties have become sophisticated in bargaining hard for their separate interests—and this complexity is time consuming. After summarizing these complex dealings in chapter 5, Goodman points out the resultant ill effects on stratification in the international economic order. While the dozen or so major third-world nations may be able to strike better deals, the approximately one hundred relatively marginal nations lack the resources or markets to attract international businesses and their efforts at control are likely to be considered as far more major obstacles for investment by TNC managers anxious to manage their time efficiently.

KENNETH E. SHARPE

Swarthmore College

Book Reviews: Comparative Politics

Principles of Group Solidarity. By Michael Hechter. Berkeley: University of California Press, 1987. 219p. $28.50 cloth, $10.95 paper.

Hechter is an "old-fashioned" social theorist in the best sense of that term. For him, theories are *"causal"* explanations that provide *"intelligible* answers to why-questions about *empirical* facts" (p. 1, original emphasis). The central question in *Principles of Group Solidarity* is a critical one: "Why do individuals in any group develop solidarity with other members in order to pursue their collective interests?" Group solidarity, when individuals act as if their own interests are served when group interests are advanced (by contributing a high proportion of their own resources to serve collective ends), is one way out of the free rider dilemma. But why should group solidarity occur?

In earlier work on the course of Celtic nationalism, Hechter answered that question "in terms of the existence of a stratification system that gives cultural distinctions political salience by linking them to individual life chances" (p. 5). He argued that "when individuals perceive that their life chances are limited merely by virtue of membership in a particular group, they will either leave that group (if they can) or come to see that they share vital interests with its members, thereby giving them reason to engage in nationalist activity" (p. 5). But in this book Hechter seeks to rethink his earlier structuralist approach by melding it with a more explicit focus on individual motivations and incentives for action derived from rational choice ideas.

Hechter is well aware of the usual critique of the standard rational choice model and sensitive to the defects and limitations of a rational choice approach. He is equally well aware of the severe problems of the standard sociological-structural approach to the problem of social order (e.g., its difficulties in explaining large-scale changes in structures of relationships and in accounting for the fact "that some people in a given structural position free ride, whereas other similarly situated actors do not" [p. 6]). He is highly suspicious of what he calls the "normative approach," which simply talks about internalized norms without accounting for why some groups are more effective socializers than others.

Hechter's claim is that he has devised "a new theory of group solidarity on rational choice foundations" and demonstrated that "far from being immiscible, sociology and rational choice are mutually complementary" (p. 8). If correct, this is a major accomplishment and this is a book to join with Mancur Olson's *The Logic of Collective Action* (1971), Albert Hirschman's *Exit, Voice and Loyalty* (1970), James Coleman's *The Mathematics of Collective Action* (1973), and Robert Axelrod's *The Evolution of Cooperation* (1984) as one of a handful of seminal works that transcend disciplinary boundaries and are major works of social science. The book reviews a tremendous range of literature in sociology, economics, political science, anthropology, and sociobiology (the bibliography includes over five hundred items). Hechter's scholarship is impressive, but even more impressive is his ability to synthesize and clarify the basic streams of research without getting lost in technicalities. *Principles of Group Solidarity* ranks with Russell Hardin's *Collective Action* (1982) as the best overall introduction to recent work on the problem of collective action. If it contained nothing more than this literature review, *Principles* would be an admirable scholarly contribution; but of course, Hechter has new insights of his own to offer, and he skillfully reworks other people's data to provide convincing empirical support for the usefulness of his model.

The key elements of Hechter's own model are *dependence* (how costly it is to leave a group in terms of opportunities forgone), *visibility* (of noncooperative acts) and *sanctioning mechanisms:* "While dependence increases the extensiveness of corporate obligations, it does not insure compliance with these obligations. Compliance requires formal controls. Some agency of the group must have the ability to monitor the members' behavior and to provide sanctions to reward the compliant and punish the noncompliant" (pp. 10–11). For Hechter, solidarity can best be understood as compliance in the absence of compensation. While none of the elements of Hechter's model are new, their integration into a model that purports to give both necessary and sufficient conditions for group solidarity is new.

For political scientists interested in party or legislative politics, perhaps the most interesting chapter is "Dependence and Party Solidarity." Hechter's training is in sociology, but he draws on data collected by political science students of legislative politics (e.g., Froman, Loewenberg, Polsby, Ripley, Turner) to explain such phenomena as cross-national variations in the aggregate solidarity of major political parties, longitudinal variations in aggregate levels of party solidarity within a given party, and so on. While the data and conclusions are not new, the development of a unified model of the factors accounting for solidarity that is able to accommodate a host of existing findings can be fitted and rich in suggestions for future research is new and exciting.

The last chapters, which deal with other theoretical issues, (e.g., moral hazard and principal-agent relations) and with practical problems of monitoring and control, lack the clean line of argument of the earlier chapters and have a certain potpourri quality. But even this weakest part of this book is rich in suggestive hypotheses, for instance, about the relationship between hierarchy and social order.

In sum, I do not think that Hechter has accounted for the genesis and maintenance of social solidarity, but he has made a major contribution. This is a book that should be read by anyone interested in how groups and organizations (and societies) function. The ideas in it are worth thinking about.

BERNARD GROFMAN

University of California, Irvine

The Welfare State in Transition. By Norman Johnson. Amherst, MA: University of Massachusetts Press, 1987. 243p. $27.50 cloth, $12.95 paper.

This is one more in the series of books published over the past decade on the welfare state crisis and in more recent years on the changing nature of the welfare state. Using the term *welfare pluralism*, this book focuses on what is increasingly referred to as the "mixed economy of welfare" and the question of what should be the appropriate roles of government, the voluntary sector, the market, and the family; and, for good social policy, what should be the appropriate balance among these institutions? From another perspective, an alternative title for the book might have been *Privatization and the Welfare State*, but that title is already in use. Obviously, with this as its focus, the book is very much at the heart of the current social policy debate. Its limitation is that it does not add anything new to the discussion, although it does provide a useful general overview of the current debate and some of the relevant literature.

Written by a British social policy scholar, the book is in fact a comparative analysis of western mixed economy welfare states, with particular attention to Britain and the United States and illustrations drawn from other countries as well. The author begins with a clear statement of his theme: that many welfare state advocates are convinced that a mixed economy of welfare is essential if the welfare state is to remain viable; that this means that the government should remain predominant as funder and regulator of social provision; but that its role as producer or provider-deliverer of social benefits and services should be shared across the other sectors. Johnson agrees that the debate about the allocation of roles across the sectors is an ongoing debate and that the balance will shift over time and across countries. He criticizes the welfare state crisis concept as being a tool of conservatives used to justify cuts in social expenditure. On the basis of extensive and rigorous examination the concept has been rejected already by many social policy analysts as inaccurate.

The author concludes his analysis by stating that "in the short and medium term the future of the welfare state appears to lie in the direction of welfare pluralism; but even with statutory finance and regulation, serious doubts arise as to the willingness and capacity of the voluntary and informal sectors to substitute for the state. If they cannot respond in the ways and to the extent expected of them by welfare pluralists, then welfare pluralism will merely serve to legitimate cuts in public expenditure and the development of market provision" (pp. 199–200). Since he does not believe that there is sufficient political will to support social policy in the long term, he sees socialism as the only viable strategy.

The first three chapters provide a general context for Johnson's analysis. He reviews the history of welfare state developments and concludes that the welfare state phenomenon characterizes all advanced capitalist societies to a greater or lesser degree. He then reviews the literature on the emerging crisis at the end of the 1970s: the economic, governmental, and fiscal problems of western welfare states and the problem of legitimation, concluding that "the rhetoric of entrenchment has exceeded its practical application" (p. 54) but that a change in emphasis has occurred nonetheless. He views the renewed interest in an expanded role for the private sectors as part of the response to the problems of the welfare state. He acknowledges that there are two contrasting perspectives in the current debate: a conservative ideology of "rolling back the state" and a pragmatic approach that sees an expanded role for the private sectors in delivering social services as part of the future development of any welfare state. The heart of the book contains four chapters on the informal sector (family and neighbors), the voluntary sector, the market, and the state—all in relation to social welfare.

He concludes with a brief review of alternatives: welfare pluralism; corporatism; and socialism, his preferred solution. This is the weakest part of the book. In particular, his concept of welfare pluralism as a theoretical construct completely distinct from corporatism ignores the fact that the private sectors play an important role in corporatist (and socialist) societies as well. Moreover, he ignores completely any mention of the growing interest in an expanded role for the three private sectors (voluntary, market, family) in many noncapitalist societies today as well. He pays very little attention to the role of regulation, although such policies are at the heart of many European welfare states today. Although most of his comparative illustrations are interesting and useful, there are several errors in his discussion of the United States. For example, he mentions the use of food vouchers with regard to the Women, Infants, and Children program but does not mention food stamps, a far more significant program; he mentions private provision of protectives services for abused and neglected children in the United States as if for-profit services were involved instead of largely voluntary social services; and he apparently does not know that the voluntary sector expanded enormously in the United States during the 1970s with public financing. Finally, although he acknowledges the validity of diversity in social policy, his conclusion seems to rest more on his personal conviction that social policy will not receive adequate political support rather than on any evidence.

SHEILA B. KAMERMAN

Columbia University

The Politics of Neocorporatism in France. By John T. S. Keeler. New York: Oxford University Press, 1987. 369p. $39.95.

Interest-Group Politics in France. By Frank L. Wilson. Cambridge: Cambridge University Press, 1988. 324p. $39.50.

Political scientists' understanding of the nature of interest group activities and their relationships with the state in modern France benefit greatly from the publication of two empirically based studies on related topics. Wilson's book is a survey of the activities of a sample of national interest groups active in Paris, while Keeler presents an in-depth study of the area of agriculture.

Keeler provides an accurate and detailed portrayal of the interactions between the largest organization representing French farmers and the state. His book is based on an impressive list of research activities during five separate periods of field work in France from 1974 through 1983 and includes discussion of the development of the dominant farmers' organization, the *Federation Nationale des Syndicats d'Exploitants Agricoles* (FNSEA), over four decades.

Keeler makes some much-needed improvements to recent theories of the putative "pluralism-corporatism continuum." Though his book does not do away with the problems of this body of literature, he makes several contributions, including a revised definition of *corporatism* (p. 9), and a discussion of "corporatization." He shows how it may be easier to aid in the creation of a dominant "partner" than to ignore one that is already in place. "Decorporatization" (pp. 252–54) is not only an awkward term but can prove to be an awkward policy, as his analysis of the experiences of the socialist government shows.

Keeler documents the important role played by the state in encouraging the growth of the FNSEA during the postwar period (and he gives more cursory evidence of state support for the major labor unions as well). His analysis demonstrates the limits of state independence, once the FNSEA had become established as the dominant organization, in the face of such a behemoth. As he quotes an official in the Socialist Ministry of Agriculture under Michel Rocard, "There is no alternative to comanagement in this sector" (p. 242).

Keeler describes the role of the state in encouraging interest groups to grow to sectoral dominance and the degree of freedom the state maintains in choosing to accept or to ignore such a dominant partner, and for that reason his study will be important to those interested in the nature of group-state relations in any country. Keeler chooses to conclude with a discussion of the "corporatist imperative" in agriculture in five Western democracies, but the evidence he presents here is weak compared to the wealth of information presented in his case study. He rightfully calls for more theoretically informed research on the nature of state-group relations, and his study is an important step in that direction.

Wilson chooses survey research over the case study and conducted a list of interviews no less impressive than that of Keeler: 99 structured interviews with interest group leaders in 1979, followed by 41 less-formal interviews in 1982 and 1984. Like Keeler, Wilson starts out by decrying recent trends in interest group research in the European democracies, rightfully noting that "neo-corporatist theorizing preceded the empirical research," a condition that over time "seems to have led researchers to assume that the presence of neocorporatism has been established" (p. 4). Nothing could be further from the truth, according to Wilson. His study shows a wide range of activities engaged in by French interest groups of all types, making him conclude that no single model explains this complexity. The pluralist model, defended by Wilson in previous writings, is found not to apply either, partly because of the strong role of the French state in controlling (or ignoring) the groups in Wilson's sample.

Wilson describes in considerable detail the major interest groups active on the French political scene and their relations with each other, in three narrative chapters. He makes no apparent use of his formal interviews in this section, though his trips to the national headquarters of these organizations obviously made him very familiar with their workings and history. The survey results presented in the next two chapters provide the first major work on the nature of group-state relations in France since a bevy of studies 30 years ago.

Wilson argues that the lack of fit between his empirical results and the theories introduced at the beginning of the book is more than just the normal "gap between the prescriptions of ideal-type models . . . and the actual world of politics" (p. 256). Because of a lack of empirical grounding, the theories are off the mark. He calls for a greater effort at empirical observation rather than theoretical postulation, and both new books on French interest group politics fit this bill. They make important theoretical contributions because of their firm empirical groundings. Reading them together is also a good lesson in the interplay between research designs, empirical findings, and theoretical conclusions.

FRANK R. BAUMGARTNER

Texas A&M University

Afro-Marxist Regimes. Edited by Edmond Keller and Donald Rothchild. Boulder: Lynne Rienner, 1987. 335p. $35.00 cloth, $16.95 paper.

This is an interesting and well-conceived book whose scholarly contributors respond to three major questions: What are the Afro-Marxist regimes and what social forces have facilitated or constrained their consolidation? What has been the relationship between ideology and public policies and the consequences for economic development? and What has been the nature and relevance of communist country presence in these states? The goal is comparative analysis; little new factual information is offered.

While noting problems in distinguishing Afro-Marxist from other self-identifying "socialist" regimes in Africa, Keller's introduction offers five key characteristics to distinguish Afro-Marxist states from others: explicit acceptance and high importance of Marxist-Leninist ideology; a central role for a vanguard party with selective membership; heavy emphasis on "developing a broad, deep, effective, and authoritative state apparatus"; serious efforts to create state control in production, distribution, and exchange; and commitment to a centrally planned economy (pp. 11–15). However, as Keller notes, planning in these states is virtually nonexistent. Efforts to create a strong state apparatus and activist state economic roles are qualities exhibited by many "populist socialist" and frankly capitalist regimes in Africa, many of which have state enterprises. The conclusion adds a sixth criterion, "the conviction that 'socialist orientation' entails finding 'natural allies' " among the communist countries, a distinctive quality often ignored (p. 284). Keller singles out "highly articulated" Afro-Marxist regimes (Angola, Mozambique, Ethiopia); three "ideologically and politically heterodox" Afro-Marxist regimes that the book does not discuss (Benin, Congo, Madagascar); and one aspirant Zimbabwe, whose leader, Robert Mugabe, and now-single party profess Marxism-Leninism. Zimbabwe's policies to date have been mildly populist. The inclusion of Benin, Madagascar, and the Congo in the Afro-Marxist grouping should have been closely argued since these regimes are unserious in Marxist policies and military in origin and leadership and only one (the Congo) has a significant party.

In general, the authors of the chapters on Angola, Mozambique, and Ethiopia note that these regimes have failed to consolidate state power and build strong stable state institutions or are experiencing severe problems. In all three countries large-scale insurgent forces are denying the government control over large parts of the country, but especially in Angola and Mozambique, where economy and society are in advanced stages of collapse. Existing state institutions have low capabilities and low political support. In Angola and Mozambique, the guerilla forces are strongly supported by South Africa (and in Angola by the United States). The authors also note clearly the avoidable internal sources of these regimes' political weaknesses. For instance, in Angola, an ethnically narrow, largely urban base for the ruling Popular Movement for the Liberation of Angola (MPLA); political underrepresentation of important rural and ethnic communities, some of which constitute the core of the armed opposition National Union for the Total Independence of Angola's base; and the reckless, rigid, centralized and inappropriate economic policies that alienated peasant producers and destroyed production for the market (p. 74-77).

There is perhaps too little analysis of the salience of ideology and vanguard parties, features the authors regard as distinctive. All authors accept that ideology plays some role in policymaking and sets some parameters on thinking. But several authors note that virtually all party members including leaders know little about Marxism-Leninism. This did not stop the formal adoption of Marxian-Leninism as regime ideology (with unfortunate consequences for political communications). Clearly, we cannot explain highly statist or collectivist measures as Marxist in origin or inspiration when many of the decision makers did not think ideologically! However, these measures did promise (falsely) increased state power for elites. Moreover, we do know that in Ethiopia Derg military leaders came to call themselves "scientific socialists" to compete ideologically with civilian Marxist groups who believed that they, not the military, should rule. We also learn too little about the parties, including range and frequency of activities and procedures. Howe and Ottaway suggest that Frelimo in Mozambique is fairly strong, but it is legitimate to wonder to what extent Frelimo and the MPLA actually exist apart from self-perpetuating high-level party bodies. We know that the Workers party of Ethiopia is a state-rigged edifice to "replace" Derg military rule, but a majority of the party's Political Bureau are the same Derg military leaders. It is reasonable to ask if the parties in Mozambique, Angola, and especially Ethiopia possess an existence apart from the state. The chapters on Angola, Ethiopia, and Mozambique observe the alienation of peasant populations from the party-state. Little noted is the suppression by these regimes of independent trade unions.

The authors seek to explain the massive economic failures in Mozambique, Ethiopia, and Angola. In general, the authors point to poor-to-terrible economic inheritances (Angola, Mozambique); poor or recklessly adopted statist and agricultural collectivist policies that sought to nationalize and manage more than the regimes had any capacity to cope with; extremely low state capacities for policy implementation; natural disasters; and the disruption and destruction of insurgent guerilla groups. Mozambique focuses on this and, unfortunately, gives little attention to government policies and economic performance.

Rothchild and Foley's conclusion is an excellent effort to draw together the analyses and evidence in previous chapters. They weigh the impact of ideology and of domestic internal and external constraints on policy selection and economic performance. While noting low state capabilities and external pressures and insurgencies, Rothchild and Foley dwell on important self-induced constraints: the ideologically legitimated concentration of power, rendering regimes insensitive to public demands and offering low opportunities for articulating interests; socially, their antagonizing the public by insensitivity to, or campaigns against, religious and ethnic groupings (ethnicity and regionalism are key bases of organized resistance in Ethiopia and Angola); and, economically, excessive (because counterproductive) efforts at regulation, centralization, and nationalization, which have generated lower levels of production and less capacity to develop and redistribute. Comparisons are made to the nonradical but more successful efforts in Zimbabwe. The authors assess the newly pragmatic responses to economic or military disaster by these regimes. The pragmatism has been limited to efforts to negotiate with hostile neighbors in order to reduce their external attacks (pursued with little or no success) and to a willingness to modify statist and collectivist policies and permit some market incentives. None of the regimes has evidenced an interest in negotiating with the guerilla movements or to share power, which in Angola and Ethiopia would greatly aid (not reduce) state consolidation and development. *Afro-Marxist Regimes* is a useful and thoughtful effort to assess the nature and effectiveness of these regimes comparatively. It also demonstrates, however, that we still know too little regarding some of their key political and institutional characteristics, in part a result of lack of research access.

JON KRAUS

State University of New York, Fredonia

Democracy, Italian Style. By Joseph LaPalombara. New Haven: Yale University Press, 1987. 308p. $25.00.

"Italy invites hyperbole," writes Joseph LaPalombara. But it is also "a place of nuance and illusion." At first glance," LaPalombara elaborates, "the picture we have of Italy is that of an ideologically polarized country engaged in an unremitting political war of all against all" (p. ix). But all is not what it seems. This is the basic theme of the book; all the rest follows from it.

LaPalombara's book completes a reinterpretation of Italian democracy that began in the 1960s, one that sees it as a system that *manages* its deep cleavages, rather than resolving them through consensus. Italy emerges from this book as a country that is extraordinary not for its excesses, but for its success in reconciling the interests of a diverse and divided population.

LaPalombara attacks the model of "polarized pluralism" that conceived of Italy as a house inhabited by an alienated citizenry divided against itself. He sees less paralysis in government than many observers claim and thinks that the multiparty system that most Italians condemn actually makes the institutions work in a fairly democratic way. He argues that the Italian political class is not the corrupt and incapable group of manipulators that is often portrayed by its critics.

Finally, LaPalombara argues that Italian democracy is in its own way highly participatory and has fostered among its people "a deep seated and widespread commitment to democratic government" (p. 19). Like the Globe theatre of Shakespeare's day, it is a participatory spectacle *(spettacolo)* in which "everyone is or can become involved in its ongoing performances" (p. 92). Beneath "the surface illusion of centrifugal conflict" there is "a much deeper harmony that is revealed to us only on further contemplation" (p. ix).

Although I am in sympathy with many of the arguments in LaPalombara's book, there are a number of troubling aspects about it.

First, regarding the model that he attacks with such gusto—that of "polarized pluralism" —by the end of Italy's fourth decade as a republic, most students of Italian politics are more likely to be struck by the similarities in the programs of the major political parties. Even more puzzling is the fact that important aspects of the conventional model—for example, that Italians vote out of identity with traditional subcultures—find resonance in the book. Recent research, in contrast, has emphasized the multiplicity of electoral motivations.

LaPalombara's concentration on the traditional model distracts our attention from the political economy. Like most observers, he emphasizes the role of politics in the economy but ignores recent work that challenges the traditional model of a politicized state whose major function is to dispense patronage. Regini and Lange's recent volume on regulation, for example, suggests a far more complex relation between state and society.

There is space here for only a few observations on the empirical propositions in LaPalombara's book. U.S. readers will find it hard to reconcile the proposition of an actively participatory electorate with the consistent alienation that appears in the surveys. Unfortunately, they will have difficulty engaging LaPalombara in dialogue, for he believes that the indexes that measure participation in other countries are out of place in Italy. This is an unfortunate lapse into the Italy-is-different school of interpretation that, in general, he abhors.

Second, on Italy's functioning as a democracy: LaPalombara is so certain of the delicate functioning of Italy's democratic institutions that he is skeptical of the efficacy of major reform. But as Gianfranco Pasquino points out in a spirited review of the book, if Italy's democratic institutions work as well as LaPalombara thinks, why is he so concerned at the danger of reform?

Third, to argue that Italy functions as a democracy does not oblige us to believe that beneath the surface lies "a deeper harmony." Those who have studied collective action, class conflict, and political protest in Italy, myself included, would have difficulty knowing where to look for this "deeper harmony." So would those Italians who have suffered either from terrorism or from the Draconian laws that have been used to suppress it.

Despite these problems, LaPalombara has done Italy and those who study it a service. By treating the country as a functioning democracy instead of a caricature, he shows that a political society can be fascinating, paradoxical, and pungent but at the same time logical, functional, and dynamic. But in reacting to the excesses of older models, he exaggerates the system's functionality, substituting only the metaphor of *spettacolo* for a more adequate model. As a result, a lively, controversial and insightful book by one of Italy's most talented observers falls short of its potential to renew the study of modern Italy.

SIDNEY TARROW

Cornell University

Grenada: The Jewel Despoiled. By Gordon K. Lewis. Baltimore: Johns Hopkins University Press, 1987. 239p. $25.00.

The overthrow and violent death of Maurice Bishop in Grenada in 1983 and the subsequent U.S. military intervention on the island profoundly shocked the people of the Caribbean. It abruptly terminated the attempt of Bishop's New Jewel movement to transform the internal economic and political relations in Grenada to give its inhabitants a more humane social environment in which to live. Gordon Lewis's latest book, aptly entitled *Grenada: The Jewel Despoiled*, examines and analyzes the causes and consequences of these tragic events.

Lewis feels that the driving force of the Grenadian Revolution was what George Lichtheim, in his essay "Imperialism," has called "populist socialism," and Bishop's speeches for the New Jewel movement are the most eloquent expression of its ideas. The historic roots of the movement are to be found in the early slave rebellions of the West Indies, which were the first manifestations of the sanguinary struggle of the oppressed against the institutionalized violence and injustice of the metropole; a violence that the imperialistic policies of the colonial power made necessary. Today the imperialism of the European nations has been replaced by that of the United States, which Lewis sees as the major obstacle thwarting a peaceful transformation and modernization of the prevailing archaic and cruel socioeconomic conditions. Also, he feels that the heroic struggle of the New Jewel movement against U.S. imperialism has placed the Grenadian Revolution in the lexicon of national liberation struggles alongside those of Haiti and Cuba. If the immediate cause of Bishop's overthrow was the harsh policy disputes and infighting among his followers and the inordinate ambition of some of them, the fundamental cause underlying all this was the nefarious, imperialistic policies of the United States.

According to the author, the four years that

Bishop led the "People's Revolutionary Government" was a period of reconstruction and change based on extensive state planning maintained within a framework of a "mixed economy" model. Besides starting to bring about the needed social alteration, the revolutionary government achieved, through adopting this model, two other important objectives. First, it gave Grenada the possibility, to a degree, to break out of the geopolitical orbit of the West by partially replacing the neocolonial bilateral commercial ties with new multilateral ones to Eastern Europe, Asia, and Africa. If in the more political aspects of Grenada's foreign policy this meant a somewhat pro-Soviet tilt, Lewis seems to wonder whether in reality this was not primarily the result of the extreme hostile attitude of Washington vis-à-vis Bishop's government. Second, the idealism and enthusiasm that Bishop's internal policies infused into the Grenadian population brought about a spirit of cooperation that could have been the foundation for a new socioeconomic structure based on true equality and justice.

Thus, to Lewis, the root causes of the problems in the Caribbean are clearly not to be found in subversive manipulations by some extrahemispheric power, but in the socioeconomic plight of the area. His belief that the problems in the Caribbean can only be tackled by a socialist approach makes him extremely pessimistic about the area's future. An imperialistic United States will always oppose this. As he sees it, there is little indication that the United States public cares to change its ideology.

Lewis's conviction that an economic elite driven by a capitalist ideology forces the United States into its imperialistic policies, which are disastrous for such weak nations as Grenada seems to ignore the general behavior of the United States in the arena of world politics. The behavior of the superpowers in global affairs can often by highly illogical, and to explain it one must examine the power relations among nations whose actions are seldom primarily based on economic motives. Besides economic imperialism, there is power politics, which can have devastating consequences for unprotected ministates and other weak, less-developed countries. Lewis, however, does realize this. He feels, therefore, that since neither the Soviets nor the North Americans are going to relinquish their strategic interests in the Caribbean, the only hope for the West Indians lies in relying on their own moral strength and intellectual insight to build a decent future for the region. This, the author believes, must be done through regional integration. Nevertheless, he feels that he must warn again that "in the Western hemisphere at least the more pressing danger to regionalism comes from the North American power" (p. 156).

This great fear of the United States makes Lewis devote a rather large portion of his work to examining and questioning U.S. motives. This, to a great extent, takes the focus away from the political scene on the island—regrettably, for it is in describing and analyzing this that Lewis is most interesting, and he is at his best in his exposition of the Grenadian Revolution: if at times he assumes too much knowledge on the part of the reader, still his immense knowledge of the area makes it possible for him to depict in minute detail and with great precision the development of the events that led to Bishop's downfall.

ALBERT L. GASTMANN

Trinity College, Hartford

Intercorporate Relations: The Structural Analysis of Business. Edited by Mark Mizruchi and Michael Schwartz. New York: Cambridge University Press, 1987. 330p. $39.50.

This edited volume contains several theoretical and empirical studies that utilize "structural analysis" to study intercorporate relations. Given the reemergence of interest in institutions and their near-equivalent structures, this volume should have been an important contribution to scholars in a number of disciplines. Two essays (Mizruchi and Schwartz; Pfeffer) present comprehensive overviews of structural analysis of firm relations in the field of sociology and discuss how it differs from the transaction cost (Williamson) and human ecology (Aldrich) approaches, which dominate in the fields of economics and sociology, respectively. Structural analysis as applied to firms is not a fully developed or unified analysis. It is divided into two major approaches: resource dependency, and social class theories (Marxist and Weberian) that attempt to extend C. Wright Mills's power elite thesis.

In the work a number of articles both theoretical (Soref and Zeitlin) and empirical (John Scott; Taira and Wada; and Fennema and Pijl) argue that the elite (capitalists) are able to unify themselves without the assistance of the state by means of interlocking directorates, social and kinship ties, and financial institutions. The only theoretical innovation over Mills's analysis is the proposition that financial institutions are the forum for resolving conflicts between various factions of capitalists. Empirically, there is a marked improvement over the previous anecdotal or genealogical evidence. Network and block models are now used to show that networks of interlocking directorships exist.

A second improvement over previous studies is the inclusion of several analyses of comparative corporate structure and interaction. Taira and Wada examine social and educational linkages of business and government leaders in Japan. Ogliastri and Davila study local elite cohesion in the context of economic development in Columbia. In the most provocative of the comparative studies, Scott reports on a 10-country study by a group of social scientists of the centrality of banks in intercorporate relations. Three distinct patterns of financial relations are observed in the countries studied. Finally, Fennema and Pijl examine international bank linkages among capitalists.

This move to comparative analysis should have made the researchers sensitive to the differences between countries. However, with the exception of Scott's piece, most look at the world with a single model in mind. For example, all the social class studies share the argument that financial institutions, especially banks, have come to be the dominant capitalist institutions in all countries. However, none of the studies note that the debt-equity ratio is extremely low in the United States and Great Britain. Equity (stocks) capital is the major source of corporate financing in these two countries. The question how banks have come to dominate industry when only 15% of corporate capital is raised through debt is never addressed.

More regretfully, none of the studies examining elite relations even attempt to relate the networks to any economic or policy effects. Mills was not able to demonstrate that the power elite was unified or had any effect on policy. In a classic study of national policy formation Bauer, Poole, and Dexter did not find any unity among capitalist factions. In these current studies patterns of interaction are analyzed with great precision but no attempt is made to ascertain if they have any effect.

Some attempt at ascertaining the effects of intercorporate networks is also necessary to differentiate the social class from the resource dependency approach. Resource dependence theories view companies as the primary actors in the business community. Corporations use their resources (power) to obtain resources from other corporations and individuals. Hence class interests, in the collective sense, need not be pursued by corporations, which should pursue their own parochial interests. Bearden and Mintz in an interesting study attempt to assess the importance the network of corporate versus personal linkages on corporate boards. However, without an analysis of the policy effects of the intercorporate networks it is impossible to distinguish the two approaches.

Several unifying themes and theses run throughout these diverse works. First, there is a near-universal rejection of methodological individualism in all its forms. The basic unit of analysis is the corporation or relations between corporations. The microfoundations of the corporations or structures, that is, relations among corporations, are at best bracketed for the time being, ignored, or rejected outright as unworthy and unneeding of study. A corollary of the rejection of methodological individualism is the rejection of the proposition that market competition or economic efficiency determines firm behavior.

This denial of microfoundations for institutions or structures, however, leaves very few options open to explain the persistence or reproduction of structures. Functionalist explanations of the persistence and dynamic aspects of structures has been a common recourse since Durkheim. However, the logical problems with functionalist explanations require an alternative explanation for the persistence or reproduction of structures. Unfortunately, nearly all the contributors to this volume, as well as those pursuing structure analyses more generally, have ignored this problem. Leifer and White are aware of it and present a novel explanation of the reproduction of structures that does not entail a functionalist logic and simultaneously present an alternative to the standard model of the market as driven by competition. Though the authors' analysis is novel and interesting, it ultimately relies on the self-actualizing expectation of corporate actors; hence, it is a return to methodological individualism through the back door.

Two other pieces present alternatives to market-determined firm behavior. Baker analyzes money as a construct of institutions or segmented markets. Palmer and Friedland examine the factors affecting the location of plants. They explicitly reject the "centrality of markets" argument and instead argue that multidivisional firm form and ownership type affect location decisions.

The proposition that capitalists can organize themselves if there are appropriate institutions is a useful alternative to the excessive emphasis on state-imposed unity so prevalent among political scientists. The state's role in different countries should vary with the ability of capitalists to organize themselves.

The shift in the focus of analysis to intercorporate business relations provides a useful contrast to the study of political institutions and unions prevalent in most of the neoinstitutionalist literature. While institutions do matter, whether or not they promote efficiency or some other maximand, for instance, power, is an open question. Several of the essays attempt to provide the beginnings of an analysis that eschews efficiency. But, as noted, these merely provide seeds for the development of an alternative analysis. Much more work needs to be done to determine whether institutions are better defined as efficiency promoters or as a historical contingent phenomenon without a maximand. Hopefully, subsequent volumes in this series will address some of these theoretical and empirical issues.

RICHARD JANKOWSKI

University of Arizona

Amal and the Shi'a: Struggle for the Soul of Lebanon. By Augustus Richard Norton. Austin: University of Texas Press, 1987 238p. $25.00 cloth, $10.95 paper.

The social mobilization of the Shi'a in the 1950s and 1960s prepared the way for their political mobilization in the 1970s under the leadership of Imam Musa al-Sadr. Then the disappearance of Imam Musa, the Israeli invasion of Lebanon in 1978, and the Iranian revolution further galvanized the Shi'a community —roughly 30% of the Lebanese population— into support for what became known as Amal (acronym for Lebanese Resistance Detachments, forming the word *Hope).* Led by Nabih Berri after 1980, Amal became the principal guarantor of Shi'a security against Palestinians in the south and against Israeli occupational forces. It opposed the rising influence within the Shi'a community of the more radical, Iran-supported Hizb Allah and represented the Shi'a in the battle for control of Lebanon hinged upon cooperation between the Maronite Christians, the most privileged group and long-time political masters of Lebanon, and the Shi'a, the least-advantaged sect, in socioeconomic terms.

That is the essence of Norton's message in a book that complements Fouad Ajami's *The Vanished Imam: Musa Al Sadr and the Shia of Lebanon* (1986). Ajami communicates the electric transformation of the Shi'a of southern Lebanon under the spiritual-political leadership of Imam Musa; Norton, who served with unifil forces in 1980–81 and returned to Lebanon in 1982 after the second Israeli invasion, studies the political consequences of that transformation between 1980 and 1985. Together their efforts help rectify the skewing of scholarly interest toward the long-dominant Maronite group and enhance our understanding of the Shi'a as the principal revisionist force in contemporary Lebanon.

The core of the book (pp. 59–124), the portion based most directly on Norton's field research, portrays the development of Shi'a hostility toward the Palestinian guerillas in the south and the emergence of a "natural alliance" between Amal and Israel. As a tacit agreement the alliance functioned to the advantage of both parties, but after 1982 Israel would not—mistakenly, in Norton's view—settle for less than a formal pact, which Amal could not accept without alienating its consistency: "Of all the blunders committed by Israel in Lebanon, those committed in south Lebanon were the most unnecessary, the least easily excused by the dumb luck of history, and, perhaps, the most far-ranging in effect" (p. 107). Amal found itself trapped into resisting Israel, even though it shared Israeli antipathy toward the return of the Palestinians to southern Lebanon and toward the spread of Syrian and Iranian influence via the growth of Hizb Allah.

The book's principal weakness is its sketchy treatment of Amal's effort to link Shi'a identity with political goals. The subtitle, *Struggle for the Soul of Lebanon,* is misleading or ironic, for although Amal's charter speaks of a spiritual enterprise, Norton portrays the movement as utterly consumed by concerns for power and security. Two appendixes provide useful translations of the charter and of a 1985 "open letter" by Hizb Allah, but Norton's cursory analysis of these documents neglects the question of their relationships to the Shi'a tradition and to Islam more generally. He notes that Amal, "essentially a sectarian movement" (p. 71), appears to contradict itself by demanding the abolition of sectarianism as the political basis of the Lebanese state. The criticism would bear more weight if he had given greater attention to Amal's interpretation of Shi'a Muslim identity and its efforts to reconcile that identity with Lebanese nationality.

Norton writes, "The key to understanding Shi'a politics lies within the realm of good social science and the range of mundane political behavior, rather than in sensationalized images of crazed fanatics willing to obliterate themselves to gain a one-way express ticket to Paradise" (p. 13). True enough, but social science cannot afford to underrate the malleability of identities and the role of ideology and religion in reshaping them.

Norton writes clearly and energetically though not elegantly. He mixes past and present tenses in describing conditions of the early 1980s, a practice that disrupts the narrative and makes the book seem outdated when, as an historical account, it is not. Much fascinating information lurks inaccessible in long, substantive endnotes. On the other hand, well-chosen photographs inserted between chapters help the reader visualize the cast of characters, and many will find the bibliography useful.

ROBERT D. LEE

Colorado College

The Politics of Development in Botswana: A Model for Success? By Louis A. Picard. Boulder: Lynne Rienner, 1987. 298p. $30.00.

This analysis posits the bureaucratic state as the primary focus for understanding the dynamics of development in the Republic of Botswana. A large, sparsely populated African country, Botswana is of interest because of its paradoxical distinction as a democracy in the geopolitical vortex of Southern Africa. The author selects four dimensions of state building that he argues both illuminate the colonial past and link it with postcolonial present: mechanisms of political control, central-local institutional relationships, rural development policy, and human resource development policy. Through historical and empirical analysis of the four patterns of behavior, Picard concludes that the dominant socioeconomic elites, at different times, have managed state structures in such a way as to maximize their autonomy (or minimize dependency) at both the national and international levels. These patterns not only link the present with the past; they also define the nature of Botswana's contemporary democracy.

The Setswana-speaking peoples of southern Africa are clustered in relative proximity in the region, deriving from intradynastic rivalries over many hundreds of years. This process came to a halt with the incremental establishment of British hegemony in the 1880s. As a British imperial venture, the Bechuanaland Protectorate had certain peculiar features, however, in that the administrative capital was actually in South Africa and the administrative officers were recruited from the English-speaking police and clerical grades of the then British Cape Colony. This arms-length approach to "protecting" Bechuanaland for the first 40 years was based primarily on the assumption that the territory would ultimately be incorporated into South Africa.

By the 1930s the British had decided to bring the protectorate more into the mainstream of imperial rule. This included an initial period of indirect rule in which traditional rulers became salaried bureaucrats and a white district administrator with supreme political authority was imposed on each district (roughly coterminous with each tribal cluster). The contradictions inherent to this formula characterized the postwar period. District commissioners (DCs) were suspicious of relying on the truly able and assertive chiefs with their own tribal staffs and customary courts. DCs were equally unhappy with unresponsive or incompetent traditional authorities whom they deigned to strengthen, so they tended to intervene more and more in all matters affecting districts. Relationships between colonial administrators and traditional authorities were strained further as a more elitist pattern of recruitment came to characterize the colonial service, with no attention to Africanization.

By the 1950s the territory was left with what Picard rightly calls a "patchwork" of political and economic structures: bits of the Westminster model, the British local government system, the paltry Colonial Development and Welfare programs, and an economic policy that served only South Africa and other European traders. This apparatus afforded political control, but it submerged any incipient political process that might generate serious policy alternatives (p. 66).

As the fever of independence spread east and south across Africa in the 1950s and early 1960s, indigenous Batswana groups began to organize. The inevitable links between Batswana nationalists and those in South Africa so worried the British that they moved very quickly to, in effect, create an alternative. This was done by establishing a national legislative council and inviting the very paramount chief whom they had exiled in 1950, Seretse Khama, to form what came to be the Botswana Democratic party (BDP). Khama won the preindependence election of 1965 easily because of his traditional leadership credentials with the largest ethnic group, national celebrity (relating to his controversial marriage), and having been blessed by the outgoing colonial officials, thus giving him the votes of the indigenous bureaucrats in the now-localizing civil service. Moreover, perhaps the most important political fact of both the BDP's initial, and continuing, success at the polls is that the rural population is widely scattered and has never really been mobilized.

In the author's view, this pattern of political-administrative dominance is reproduced by the relationships between local council members in the district councils and their permanent administrative staffs, which are a separate statutory service corps from the civil service. Although his principal claim is that local council politics are irrelevant to national policy-making, he concedes that there are rivalries and status differences between local government officers and the central civil service that make for a less-than-totally-unified administrative class. Whatever rivalry lies between local and central civil servants, however, is compensated for by the dependence of councils on the central treasury for both capital development grants and funds to cover operating deficits.

The author likely overstates his case. Twenty years is not very long in terms of the institutionalization of effective local government in a country where the population was without the possibility of officially supplied secondary education until after 1966. The quality of both local councilors and permanent staff has improved notably in that period. Moreover, if one reflects on Picard's own evidence in the domain of human resource development spending, the fact that the central civil service has obtained the lion's share of human development resources has palpably worsened the relationships between central and local administrators. If this stratification is combined with the possibility that some municipal and local councils will have opposition majorities in 1989 (to go along with their victory in the capital city in 1984), it could have a political effect on central-local relations in which local staff are allied with their own councils against the center.

To be sure, rural development policy reflects the convergence of interests between the political and administrative elites. Such policies have tended to emerge after near-crisis conditions obtained and what was intended to be redistributive policy, in the event, became distributive. Land reform related to livestock development came after the range was in grave danger of depletion from overgrazing and water points were already disproportionately in the hands of the larger cattle owners. Subsequently, arable agricultural policy came along in the late 1970s only after it became clear that most Botswana did not own cattle after all and would need to farm for their survival.

Another policy domain that would also validate the underlying thesis is that of industrial and commercial policy. Curiously, there is no such treatment. It is the purely fortuitous diamond discoveries and revenues that have compensated for the collapse of beef exports during the extended periods of drought and animal disease. A combination of luck and commercial policies enabled the Botswana elite to survive its failed rural development policies.

Devoting the vast majority of space to the evolution of political structures and center-periphery relations makes for somewhat of an imbalance between the analysis of structure and that of the different policy areas. There are also varying degrees of empirical depth, between the solid treatment of public service staffing on the one hand and rural development policy on the other.

I would argue that Picard's position, despite his claims to the contrary, is now the orthodox explanation of Botswana's "democracy." However, his larger contribution is to illuminate well an actual case of state developmentalism (as opposed to the more common state welfarism and eventual state decline in Africa). Missing from this analysis, however, is the vitality and grass roots participative ferment going on in both the urban *and* selected rural areas, which are challenging the institutional order so well elaborated here.

E. PHILIP MORGAN

Indiana University, Bloomington

Ministers and Ministries: A Functional Analysis. By Richard Rose. New York: Oxford University Press, 1987. 287p. $20.00.

Departments, agencies, and bureaus are in an important sense the building blocks of modern government. The great bulk of governmental activity is channeled through them; and it is likewise through them that most citizens have the majority of their contacts with government. The outcomes of the struggles among departments and agencies often determine, or at least influence, the content of public policy. Not least, the heads of government departments in many countries, though not in the United States, are precisely those countries' most powerful politicians. Yet government departments and agencies, with the notable exception of the U.S. State Department, have oddly been little studied by political scientists.

Richard Rose has therefore performed a valuable service in identifying government departments as institutions that are crucial to both government and policies in Britain and, further, in relating the "governmental" life of departments to the "policies" lives of the career politicians who head them.

Ministers and Ministries consists of four chapters by Rose in which he categorizes Britain's departments according to their longevity, their internal structure, their political status, and the resources they command. These are followed by three chapters, written by collaborators, on the United Kingdom's three "territorial" departments: the Scottish, Welsh and Northern Ireland Offices (all of which administer an unusually wide range of government programs). Then Rose himself completes the enterprise with a series of discursive reflections on, for example, the role of the undersecretary in the British administrative system and the differences among administrative structures in Britain, the United States, France, Sweden and Germany.

The approach throughout is largely descriptive, and most students of the British system will think that the tables alone (for instance, on the size, organization and spending of departments) are worth the price of admission. But certain general themes emerge. One is the sheer variety that exists in a system that from the outside can look grey and monolithic. Another is the almost random relationship that Rose finds between the political importance of departments and the size of their budgets. The author enjoys poking fun at the current British penchant for managerialism, largely inspired by Margaret Thatcher. He points out that cuts in actual programs are hugely more important as a means of cutting government spending than are savings derived from greater efficiency. He points out, too, that being a good manager and being a successful politician have precious little to do with each other. Margaret Thatcher, politician, is a brilliant success; Margaret Thatcher, manager, does not bear thinking about.

Ministers and Ministries contains a great deal of useful material. If, nevertheless, it leaves the reader feeling slightly hungry—as after a filling but not altogether nourishing meal—it is for three reasons.

First, and least important, the presentation, although invariably clear, is also wordy and somewhat repetitious. The author not only repeats what he has already said in these pages, he repeats what he has said in his own earlier writings (so that roughly a sixth of the references in Rose's bibliography are to Rose). He also has a tendency to belabor the obvious, with large numbers of sentences like, "To meet responsibilities, a secretary of state needs help."

Second, Rose's use of quantitative data, while often imaginative, is also sometimes disconcerting. Thus, in Table 4.2 he sets out four measures of departments' political status. The measures themselves are probably valid; but the data are drawn, in column 1, from the period 1945–80; in column 2, from 1980–84; in column 3, from 1945–85; and in column 4, from a single point in time, apparently early 1984. No effort is made to explain or justify the use of such disparate data points (some of them not even overlapping). Moreover, the four columns, drawn from such disparate time periods, are used in Table 4.3 to produce a summary ranking of the political status of departments as of the late 1980s. The ranking may well be accurate; it probably is. But the methods used to construct it raise questions in the reader's mind that might not otherwise have arisen.

Third, and most important, the author frequently makes statements that are probably true and that, if true, have important implications; but he makes the statements without either producing supporting evidence or setting out what the implications are. For example, he says flatly, and probably correctly, "The prospect of frequent [cabinet] reshuffles discourages a Secretary of State from becoming deeply involved in the work of the ministry, for it is only momentarily 'his' " (p. 83). This statement, if true, is pregnant with consequences for the whole system of government in Britain. But the reader is given only reasons suggesting why the statement *might* be true, none suggesting that it actually *is* true. Here, as elsewhere, the reader is left feeling vaguely dissatisfied. The very strengths of *Ministers and Ministries,* which are considerable, consistently draw attention to its limitations.

ANTHONY KING

University of Essex

Parallels and Actuals of Political Development. By A. H. Somjee. New York: St. Martin's, 1986. 141p. $27.00.

The corpus of theoretical knowledge of the modernization school continues to influence and shape development policies of most non-Western societies. According to this school, there is a historical continuity between development in advanced Western countries and that of the new states. Presumably, the relationship between the two will benefit the latter through the diffusion of resources like capital, technology, and cultural values. The concept of development is thereby measured against the degree of industrialization paralleling that of Western societies. Thus, the West is seen as the center of global revolution for modernizing, non-Western states. Among the popular works of this school are Banfield, *The Moral Basis of Backward Societies* (1958); Lerner, *The Passing of Traditional Societies* (1958); Hunter, *Modernizing Peasant Societies* (1969); and Parson, *Structure and Process in Modern Societies* (1960).

The book by A. H. Somjee, *Parallels and Actuals of Political Development,* explores this pervasive body of theoretical knowledge. Somjee's main argument is that the accumulated knowledge of the modernization theorists from which political development emanates is fundamentally flawed in its assumptions and logic. It shows little regard for the diversity of humankind and consequently overlooks the traditions, history, and culture of societies within which the process of development takes place. The purpose of the book is to "identify some of those inadequacies, insensitivities and distortions" so as to "understand the social and political reality of the non-Western world" (p. vi).

Somjee discusses the major "biases" of Western social theorists and concludes that because they assume the process of modernization will do for non-Western societies what it did for the Western world, their conceptions and perceptions are biased. Their exclusive focus on the West has led to the projection of Western values as the universal standard by which others are measured. The ethnocentric bias of such a conceptual approach has led to a general dichotomy between tradition and modernity as exemplified in the works of Alex Inkles, David Smith and Talcott Parson. The author shows further that neither the functionalist approach by Pareto and Radcliffe-Brown, the structural-functionalism of Parson, nor the Parsonian reductionism that influenced Gabriel Almond were sensitive to the uniqueness of different societies. Similarly, the psychological and diffusionist approach, much in evidence in Lucian Pye's *Politics, Personality, and Nation Building* (1976), neglected the traditional base of the Burmese people.

This lack of sensitivity and the relativist position taken by these theorists have created an "oversimplified view of the actualities of political development" (p. 25), argues Somjee, a shortfall that has resulted in a failure to "grasp the extent of diversity among various political societies and the actuality of their development process" (p. 30–31). Anthropologists once faced similar problems, but as he points out, they dealt with it forthrightly by examining "specific aspects of human behavior and the operations of social and political institutions in situations which are deeply influenced by different cultures" (p. 30).

Chapter 2 acknowledges the pioneering works of earlier thinkers like Tocqueville, Weber, Schumpeter, and Bendix, in recognizing human diversity. The validity of their approach is illustrated with the experiences of some non-Western societies of South Asia, Africa, the Middle East, and Latin America. To reinforce his case for careful consideration of the complexities of development, Somjee devotes chapter 3 to a discussion of India, China, and Japan, "societies with several layers of culture, history and tradition in them" (p. 61). These "multilayered" societies have apparently succeeded in their development efforts because they selectively "borrowed" ideas that fit within the peculiar context of their traditional institutions.

The author calls for the refinement and reformulation of theoretical approaches that avoid the trap of searching for "parallels with a world that is familiar to us in a world that is relatively less familiar" (p. 111). The challenge for theorists is to address the "need for relativism in understanding the context-dependent aspects of institutions and political behavior of people" and the "need for universalism for evaluating and judging those areas of public concerns which affect everyone" (p. 117).

Certainly, Somjee's book is a welcome addition to the gradually accumulating literature on unconventional thinking on development. It provides deeper insights into the sometimes illogical assumptions of the dominant school of thought. However, some weaknesses are evident. The author leaves the reader with a hazy understanding of political development, the dominant concept of the book, which he treats only sparingly in the last few pages.

In other ways as well he raises unanswered questions. His concept of public minimum (the rule of law and basic human freedoms) is a welcome answer to the problem of political development, but is the institution of a public minimum possible in societies described by the author himself as lacking political capacity? Furthermore, is the concept compatible with non-Western political culture? The question of political change is central to the actualization of a public minimum. Should we hasten change or allow history to take its course? Somjee seems to support the latter when he says that those who try "to force the pace of change" engage in "century-skipping" (p. 35).

Another weakness is the absence of a discussion on the economic context for the public minimum. There is a symbiotic relationship between *political* and *economic* development. Since the idea of rule of law and human freedoms originally developed to support private property ownership and capitalism as a whole, do we assume that political development in non-Western societies is possibly only via this economic model?

Many readers also may find his proposal for political development troubling, one that balances the needs for relativism, which enables us to understand differences in societies, and universalism, which enables us "to emphasize the basic moral and public minimum without which civilized political life becomes impossible" (pp. 124–25). The prescription for universalism appears contradictory to the call for cultural relativism. Such a dualistic position is logically inconsistent, since it undercuts the basic premise of the book's thesis.

However, these extenuating factors pale in significance when measured against the author's skill at exposing the epistemological flaws in the Western concept of development for undeveloped societies. He is both insightful and masterfully concise in laying out the key weaknesses in the dominant corpus of theoretical knowledge. It is must reading for anyone seeking to understand the problem of political development in non-Western societies.

BAFFOUR AGYEMAN-DUAH

Bennett College

Elites and the Idea of Equality: A Comparison of Japan, Sweden, and the United States. By Sidney Verba, Steven Kelman, Gary R. Orren, Ichiro Miyake, Joji Watanuki, Ikuo Kabashima, G. Donald Ferree, Jr. Cambridge: Harvard University Press, 1987. 323p. $30.00.

This book is a sequel of sorts to Verba and Orren's *Equality in America* (1985) and a remarkable tour de force about the classical, Rousseauean question about inequality, here judiciously directed at elite preferences and realities in three different industrialized democracies. The authors' argument is complex and has some flaws in logic and execution. Nevertheless, this book is most likely to kindle lively discussion on the theory and method of measuring and explaining economic and political equality in a variety of settings.

Purists may object to some of the particulars of the large elite samples that the authors have assembled. For example, why there are no Swedish bureaucrats, U.S. professors (actually, some of the "intellectuals" gathered from *Who's Who* turns out to be professors, Swedish or U.S. protest groups, or other disparities among the national samples? It may well be more a matter of insufficient information being given the reader (pp. 61–62 and appendix) rather than serious omissions. As the authors argue, an understanding of equality in modern societies requires in-depth analysis of popular and elite values cross-sectionally, historically, and in the context of key groups ranging from such established majority groups as the business and labor organizations, the media, and the bureaucracy to challenging minorities and protesters. The varying fit of these visions of economic, political, and gender equality into the major partisan alignments and the face-off between labor and business largely accounts for the distinctive patterns of equality and government policy regarding inequality in the three nations today. The deferential attitudes of the Swedes and Japanese, for example, have permitted strong governments to pursue a modicum of egalitarian redistribution even though the resulting patterns of income inequality still place Japan much closer to the United States, while Sweden continues to be a beacon of economic and gender equality, where, ironically, all elite groups today prefer to increase rather than decrease the comparatively small income gap, presumably to afford the more productive people a better economic incentive. In Japan and the United States, where reported pretax incomes of business executives and elevator operators vary in a ratio of 30:1, most groups express a willingness to reduce the gap and to raise the bottom levels if not to impose a cap on the individual capacity to earn higher salaries. A factor analysis of 25 equality issues among these elites discloses five major dimensions of which at least three clearly set off the United States elite consensus from the rest. Even the challenger groups in this country reject radical redistribution, and are lukewarm in their support of the welfare state and unenthusiastic about giving women more access to power for reasons that are explored thoroughly in chapter 10 on gender equality.

The authors are least persuasive on the implications of political equality issues and their interplay with economic issues. Few will object to measuring political equality in terms of stratified political participation (pp. 13–14) and of reputation, as reasonably objective estimates. But gauging elite groups by juxtaposing their placement of themselves and others on a scale and their placement of where they and others *ought* to be—naturally business and labor in all three countries each place the other much higher and then insist on the other's demotion—left me with a vision of clay feet placed in sand. The authors then deduce from this that political inequality is "usually a zero-sum game," in which the haves cannot afford to share part of their influence with the have-nots because the latter will demand it all (and because there is neither a metric for influence nor any certainty of just how much the established elites may have and keep) and the available pool of power units is not likely to grow. This very different dynamic of possible redistribution of political inequality, they suggest, is strikingly at odds with the inherent logic of economic inequality, which does permit everyone at once, at least in a growing economy, to gain or to work out satisfactory and non-zero sum compromises. As dubious assumption is piled upon sweeping generalization, the clay feet begin to sink ever deeper into the quicksand. Is it really true that influence cannot be divided and shared in a large polity and that the understandable desire of the less influential for more clout must threaten a radical redistribution of power? Is it even true that an "expanding polity" could only be one "where the power of the state and the range of its activities were expanding" or a conflict-laden "pluralist [group] policies utopia" (p. 211) rather than simply a process of democratization—or a newly decolonized nation-state—that might facilitate a broader distribution of influential roles without a revolutionary turnover of the powers held by the established elites? How did the U.S. polity make it through successive waves of democratization without a revolution?

There are some smaller quibbles as well. Are there really no "ascriptive groups" challenging the Swedish system (p. 87) or would immigrants (p. 102), Finns, and smaller groups qualify? Bismarck's welfare legislation certainly did not precede but followed (by a decade and more) the introduction of universal male suffrage. It was obviously an attempt to appeal to working-class voters that might otherwise have followed the lure of socialism (p. 267). The U.S. business elite likewise ought to have a better excuse for its unwillingness to "extend more political rights to the disadvantaged" than to plead its own underestimation of its own power (p. 270). Finally, the authors rather overstate the "cut back [of] welfare services" except for "minimal, governmental welfare functions" by the Reagan administration and understate the "modest redistributive effects of the Great Society Programs" (p. 273). Except for marginal programs and grand gestures, the Reagan revolution did not appreciably reverse the quantum jump of income transfers, in particular, to the elderly, of Johnson's Great Society with Nixon's additional improvements.

PETER H. MERKL

University of California, Santa Barbara

INTERNATIONAL RELATIONS

Beyond American Hegemony: The Future of the Western Alliance. By David P. Calleo. New York: Basic Books, 1987. 288p. $20.95.

The state of the Atlantic alliance is a matter of perennial debate among scholars and policy makers, despite NATO's rather remarkable durability. Issues of burden sharing, the reliability of the United States' commitment, alliance scope, and relations with the Soviet Union have periodically strained ties and fueled concern.

Although both books under review were written before the conclusion of the Intermediate Nuclear Forces (INF) treaty, they address issues relevant to the current debate. Both provide brief overviews of the evolution of postwar U.S. policy toward Europe and of recurring issues in alliance relations. Where Cyr affirms the viability of managing those relations with some organizational tinkering, however, Calleo avers that fundamental change is necessary for the sake of the United States, Europe, and the international system. He makes a strong case for devolution to the Europeans of primary responsibility for both conventional and nuclear defense.

Beyond American Hegemony is not just a book about the need for Europeans to bear a greater share of the burden of their own and the West's common defense. Calleo starts from the premise that NATO has been "the nucleus of the postwar international system, . . . the vital center of military stability and politico-economic order" (p. 3). Given global changes and the relative decline of U.S. power, he argues, "attempts to perpetuate America's old role increasingly damage the international system and the United States itself" (p. 4). What sets his book apart from other treatises on the alliance is the examination of NATO in a larger context of the global economic system and the linkage between the economic health of the United States and security.

Calleo's point of view is clear, his treatment lucid, and his effort to relate a complex set of elements largely successful. The book is rich in insights and provocative analysis, but Calleo does not always succeed in weaving the pieces tightly together, most notably his discussion of U.S. fiscal policies and politics, which he likens to "a never-ending Boston Tea Party" (p. 110).

Calleo examines the requirements for devolution in both conventional and nuclear forces, the latter—through inherently difficult to contemplate in political terms—being essential if the Europeans are going to assume responsibility for their defense. The European Community's success in achieving confederal, multilateral cooperation in Europe provides a potential institutional context for devolution, he argues. There has been a tendency to denigrate these achievements particularly among political scientists, who, he suggests, "have never forgiven Europe for not living up to integration theory. Their disillusionment has unfortunately spilled over into the general public" (p. 173). Much as it has driven European economic integration, Franco-German cooperation would be at the heart of any movement toward military cooperation, which is not, Calleo argues, inherently more difficult than economic cooperation. To this end, Calleo provides superb, brief reviews of French and German policies in the political, economic, and military spheres. What neither Calleo nor Cyr notes—because discussions were only beginning—is that Franco-German military cooperation picked up momentum as the INF treaty moved to conclusion and proposals now exist to link a revitalized Western European Union to European Political Cooperation as the basis for European defense cooperation.

Calleo does not neglect to consider how the Soviet Union might react to devolution. He leaves the reader puzzled, however, by noting that the status quo in Europe serves Soviet, even more than U.S., interests. If so, one wonders with him whether, if both superpowers stand to lose in a more plural world, they shouldn't cooperate to prevent it (p. 205).

That more plural world is the result of the success of U.S. policies that led to European and Japanese recovery, political independence and economic growth for the third world, and Soviet containment. Walter Lippmann warned 40 years ago of the dangers of policies that did not define the scope of U.S. interests and resources. Calleo's arguments are a contemporary echo of those concerns. Specifically, he charges, "America's oversized military commitments, of which NATO is the biggest single component, have pressed the United States into fiscal and financial practices destructive to American, European, and global prosperity" (p. 215). Calleo's conclusion is that military spending must be controlled to solve the United States' budget problems. That requires change in NATO commitments. Although devolution was a logical alternative at other points in the past, Calleo suggests, neither the United States nor the Europeans wanted to change the comfortable arrangements by which Europe had become dependent upon the U.S. protectorate. Why, one might ask, short of fiscal collapse, should they want to do so now?

Calleo leaves the reader wondering about the consequences of devolution in the Atlantic alliance for other U.S. commitments, most notably Japan. Likewise, once the fiscal house is in order, what might be done with the "devolution dividend"? Calleo argues that change must not be sudden or traumatic, but he offers little in the way of strategy. Critics will quickly ask what would happen if the Europeans do not accept devolution. However, no book that attempts to be provocative is likely to provide answers to all questions Calleo does a great service to the debate over the state of the alliance as well as to that over U.S. fiscal policy by his effort to shed light on the complex, interrelated set of geopolitical, economic, and military factors and to propose a fundamental change in U.S. policies and in U.S.-European relations.

"The case for the status quo is a powerful one," notes Calleo. "Every decade the alliance has managed to meet serious challenges without making fundamental changes" (p. 4). This success is precisely the basis on which Cyr argues for maintaining the status quo with only a bit of tinkering with organizational or administrative arrangements. His vision of a "frozen situation" in Europe, while not strikingly different from Calleo's, nonetheless lacks the broader, global perspective of the emergence of a more plural world and of NATO as more than just a military arrangement between the

United States and Western Europe.

U.S. Foreign Policy and European Security is not a book about NATO per se nor really is it about European security. It is an essay on U.S. foreign policy, emphasizing NATO and U.S. relations with the three principal European members. In reviewing the recurrence of many issues in what he calls the "ancient" alliance, Cyr dwells in some detail on the Eisenhower and Kennedy-Johnson years, touching also on non-European issues that impinged on Atlantic relations, on policy-making processes, and on the differences in the degree of interest and approach of each administration to those relations. Although Cyr asks at the outset whether NATO's scope should be expanded to include economic policy coordination, he gives little attention to economic aspects of either U.S. policies or European security. Many readers may find Calleo's arguments about U.S. manipulation of its international monetary position to compensate for internal problems overdrawn. Yet one could wish for more consideration by Cyr both of economic issues in alliance relations and the link between military spending related to NATO and fiscal health.

Doubtless there is no exit from periodic crises in Atlantic relations, let alone the strains of European dependence on the U.S. protectorate. Although Suez remains perhaps the most serious crisis to affect the alliance, as Cyr suggests, Calleo presents a strong case that economic strains, even more than the perennial political and military strains, have a cumulative effect for both and for the international system. Where Cyr is optimistic about the continuing ability of the United States and its allies to manage those strains because it is in their mutual interest to do so, Calleo's case for devolution is an argument that management of the alliance cannot resolve the economic and military problems engendered by the decline of the United States' hegemony and the weakness of its fiscal and monetary policies. To change or not to change, that is the question. The answer depends on how much one thinks the world itself has changed and hence requires rethinking old commitments and relationships.

MARGARET P. KARNS

University of Dayton

Empires. By Michael W. Doyle. Ithaca: Cornell University Press, 1986. 407p. $42.50 cloth, $14.95 paper.

Empires and imperialism, Michael Doyle argues in this important and illuminating book, have been vaguely defined and poorly understood. *Imperialism* has meant everything from formal annexation to foreign policy influence or economic superiority; it has been explained, variously, by events or dispositions within the imperializing metropole (Hobson, Lenin, Schumpeter), by the weakness of the imperialized periphery (Gallagher and Robinson), or by the pursuit of advantage within a system of power-maximizing states (Taylor, de Romilly, Benjamin Cohen). In contrast, Doyle distinguishes empire as "a relationship, formal or informal, in which one state controls the effective political sovereignty of another" (p. 45), including both its domestic and its foreign policy; and he insists that it can be explained only by a combination of "metrocentric," "pericentric," and "systemic" elements.

Specifically, Doyle contends, three factors are separately necessary and jointly sufficient (p. 130) for the rise and survival of empire: (1) a metropolitan society that is both socially differentiated and politically integrated; (2) a periphery that is either socially undifferentiated (e.g., a tribal society) or weakly integrated (a patrimonial, feudal, or ethnically fractionated society); and (3) some "transnational extension" of the metropole—economic, religious, or political—that creates bonds of common interest and hazard between metropolitan and peripheral groups. The specific form that imperial domination takes—formal or informal, direct or indirect—depends on characteristics both of the peripheral society and of the prevailing international system. The weaker and less differentiated the peripheral society and the more multipolar the competition among metropolitan powers, the likelier is the imposition of formal, direct rule.

Doyle devotes part 1 of the book to developing this theory, not deductively "from basic axioms about human behavior" (p. 13) but through reflection on historical cases: Athens, Sparta, and Rome; and the Ottoman, Spanish, and English empires of the early modern era. In part 2, he endeavors to apply the theory to the preeminent recent case, the nineteenth-century "scramble" for Africa.

In clarity and testability, Doyle's theory is a considerable advance, but it is largely refuted by the facts. None of his three conditions is necessary for the rise or survival of empire as he defines it. First, not only highly differentiated societies become imperial metropoles. Doyle concedes (pp. 105ff.) that the early Islamic empires were won by tribal (i.e. undifferentiated) societies; the Persian empire was patrimonial, that of Charlemagne was feudal. Neither is domestic unity required. Doyle himself, having repeatedly insisted that unity is essential, argues that Third Republican France pursued empire as an answer to political *dis*unity and weakness (pp. 314–18). Second, not only undifferentiated or poorly integrated peripheries lose "effective political sovereignty" to other powers: the Ionian Greeks of Asia Minor were conquered and ruled by the less-differentiated and less-integrated Persians; the Carthaginians were at least as socially advanced and cohesive as the Romans; the East European societies were hardly less articulated or integrated when the Soviet one to which they lost effective sovereignty after World War II. Third, unless "transnational extension" is defined tautologically (as one might infer from Doyle's discussion at pp. 129–130), it, too, is not required. The Spaniards in Mexico and Peru had no prior contacts with the peoples they conquered; and ancient imperialists, bent on extermination or enslavement, often developed few.

Nor do all three conditions, taken together, suffice to assure the rise of empires. Nothing reveals this so clearly as Doyle's own part 2. In nineteenth-century Africa, two of the three theoretically sufficient conditions were already in place: Europe had socially differentiated and politically integrated nation-states; African societies were, with some exceptions, tribal. The growth of "transnational extensions" from European to African societies should therefore have sufficed to unleash formal, direct European imperialism in Africa—something very like the "scramble."

This would be a difficult case to make, and Doyle's part 2 does not attempt it. It offers an almost conventional, if impressively learned and balanced, historical narrative, in which the central variables of part 1 recede almost into obscurity; the "scramble" is explained principally by the international system and the vagaries of domestic politics (see esp. pp. 344–49). French imperialism is attributed to Bismarck's machinations and elite insecurity, German imperialism to Bismarck's sly (and brief) effort to subvert an anglophile crown prince, British imperialism to electoral and coalitional tactics. In each case, the decline of British hegemony plays a role. In sum, however, we are left with the rather tepid conclusion that "no one explanation was sufficient. . . . The foundations of empires remained a combination of causes" (p. 341).

Doyle has produced, despite his nods to theory and his use of terms like *comparative statics,* an old-fashioned work of historical reflection and induction, informed by great erudition, a seemingly total mastery of historical sources, and a brilliant prose style. If he has failed to explain imperialism, he has renewed, sharpened, and advanced our study of it. This is a contribution on which others will build. Despite its flaws, or perhaps because of them, it is a volume that no scholar of comparative or international politics can safely ignore.

RONALD ROGOWSKI

University of California, Los Angeles

Woodrow Wilson and the American Diplomatic Tradition: The Treaty Fight in Perspective. By Lloyd E. Ambrosius. Cambridge: Cambridge University Press, 1987. 323p. $34.50.

Books on Wilson are bound to be complex, given the subject and the time, and Ambrosius' book is no exception. But there is a clear focus of both time and theme. Ambrosius begins by following the development of Wilson's conception of the League of Nations, starting with Wilson's sequential reactions to the war in Europe and the various possible roles in this miserable episode for the United States. We get a good look at U.S. diplomacy, largely carried out by Colonel House, before U.S. entry into the war. Of particular interest are U.S.-German relations and Wilson's apparently genuine effort to maintain U.S. neutrality, which was eventually and definitively undermined by the initiation of unrestricted submarine warfare by the Germans. The ambiguity and incongruity of Wilson's perceptions of the world and the United States' role in in it emerge not merely as the product of Wilson's innovative and restless mind but as the product of a U.S. tradition that stresses universalism as well as nationalism, control as well as—even more than—order. Wilson wanted to reshape the world, and the league and the notion of collective security were together the means by which this could be achieved. Wilson might be given low marks for realism, but it is interesting to note that his scheme, however pretentious and unjustifiably optimistic, was based on what at least he believed to be low risk and low commitment for the United States. In some respects, at least, Wilson appears a rather cautious crusader.

Ambrosius' study takes us through the Paris peace conference (not many surprises here), the treaty fight in the Senate, and the presidential election of 1920. Throughout the book we watch the truly great and truly tragic figure of Wilson grasping for a better way in the world without at the same time being able to grasp the specifics of this better way or of how to get there. One of the themes of the book that will be of particular interest to political scientists as well as diplomatic historians is the importance of operating within a consensus. It is rather striking that Wilson, faced with a Senate narrowly controlled by the opposing party and needing a substantial two-thirds majority to support the treaty, did so little to build the necessary consensus. The case study of how to build and maintain a consensus is best demonstrated by Senator Henry Cabot Lodge and former senator and secretary of state Elihu Root, two significant figures whose views of the world may have lacked Wilson's revolutionary vision but might well have been incorporated into Wilson's position. It was their views that formed the basis of the Republican party position, with almost predictable results in the 1920 election.

Indeed, one of the most useful contributions of Ambrosius' study is the careful attention given to Root and Lodge. At least by present day standards they do not appear here as obstructionists but as responsible public servants. Root, a Republican supporter of the league, might well have provided the guidance and support Wilson most needed. His preference was for a more restrictive, more precisely fashioned covenant than Wilson had in mind, something, in fact, rather more like the United Nations in some significant respects. The irony is that both Wilson and Root were reformers and perhaps inevitably a step ahead of their time. Greater than the tragedy of Wilson's characteristic self-destructiveness is the tragedy that two good, forward-looking leaders could not have collaborated throughout the entire episode.

It would have been helpful to the nonspecialist reader if major actors had been given more complete introductions, for it is unavoidable in books on politics and, in particular, diplomacy that there is more than the usual number of people to contend with; a narrative score card would have been appreciated. This quite minor criticism should not in any way detract from the book, which will, in fact, be of greatest interest to scholars in diplomacy, the presidency, and public policy. It is extremely well researched, and the material is presented neatly and coherently, so that the story flows well. The book includes a substantial bibliography (one work that could have been added in the memoir section is Harold Nicolson's *Peacemaking Nineteen-nineteen [1984]*, and this should be especially useful to both experts and serious students. Altogether, Ambrosius' study is an important contribution to our understanding of this complex and critical era of U.S. foreign policy.

BARCLAY WARD

University of the South

Confronting Revolution, Security through Diplomacy in Central America. By Morris J. Blachman, William M. Leogrande, and Kenneth E. Sharpe. New York: Pantheon Books, 1986. 438p. $22.95. cloth, $12.95 paper.

Grappling with instability in the Caribbean and the prospect of intervention in the Dominican Republic, President Kennedy succinctly characterized the central dilemma of U.S. foreign policy: "There are three possibilities in descending order of preference: a decent democratic regime, a continuation of the Trujillo regime or a Castro regime. We ought to aim at the first, but we really can't renounce the second until we are sure we can avoid the third." *Confronting Revolution* centers on this intractable dilemma.

The book is the outgrowth of the extensive and exhaustive foreign policy debate that began in 1982 between 15 country experts and foreign policy experts on Central America. First as a nonpartisan working group and then as the authors of this study, the goal of the contributors was an "objective" analysis of U.S. policy in the region and the formulation of "sound" policy alternatives. The authors and editors of the 14 selections believed "that ignorance of reality tempted policy makers to substitute rhetoric for analysis" (p. vii). Instead of cold war ideology and a costly and counterproductive policy of hegemony, they proffered "principled realism" (p. 351).

The foremost strength of this study is first-rate analysis throughout. The authors methodically assess generally unexamined assumptions and principles of U.S. foreign policy: idealism-realism, superpower rivalry, the "East-West optic," order or structural change, the domino theory, dictators of the Right versus leftist governments, poverty or subversion as causes of revolution, and military muscle versus diplomatic ingenuity. The case studies in the book provide carefully weighed facts and prove the central conclusions of the analysis, that direct or indirect use of force and exclusion of leftists from power has been counterproductive to "real" national security and that hegemony is not synonymous with the national interest.

The essays in this study provide an extremely thorough look at all aspects of the Central American turmoil: the origins of the crisis; an in-depth country analysis of the nations in the region, including Panama; the role of other actors who have tried to influence events, such as Cuba, the Soviet Union, and the Contadora group; and the role of the United States. Each delves into the rich complexity of foreign policy decision making, the bureaucratic infighting, the partisanism, and the domestic and constitutional constraints, which radical critics tend to simplify. This book exposes the errors of Reagan's Central American policy in a manner that cannot be ignored by the policy elite or the U.S. public as partisan or ideological. The acceptance of such criticism as fact is long overdue.

Although most of the current literature, both radical and liberal, critical of the Reagan administration's policy in Central America has marshaled similar facts and conclusions, these arguments need to be asserted again and again in the hope that they may be acted upon. But this is precisely the problem. The authors assume, as most liberal literature does, that once errors are exposed corrective measures will be taken, eventually. It is merely a matter of some clear thinking and talking, some partisan bargaining, and a little bureaucratic infighting. If there were an objective and fully representative ideological continuum in this country, the analysis presented in *Confronting Revolution* would fall dead center on that imaginary line, but after the last eight years of Reaganite reaction, the present study appears radical, even leftist. It seems ironic that the study pleads that the Left be permitted meaningful political participation in Central and Latin America, while the "Left" in the United States is marginalized.

The validity of the prescriptions in this book depends on what the fundamental nature of the U.S. regime is. If, as this book assumes, the United States is basically well intentioned but unable or unwilling to see its true long-term self-interest, the book's policy conclusions are relevant. On the other hand, if the United States is a reactionary, capitalist empire in decline, controlled by an economic power elite or military-industrial complex that intends to maintain its preeminence and hegemony abroad no matter what and a facade of democratic government at home, what the authors conclude may be irrelevant. After all, if similar prescriptions have been made before, why has so little changed? Differences that exist are inherently about the "how" of control. In the authors' words, "Whereas Carter's containment strategy was essentially political, Reagan's was essentially military—but both sought the same result" (p. 327).

The nature of the U.S. system may indeed be beyond the purview of this excellent study of contemporary U.S. foreign policy in Central America. Still, until we understand the nature better, and have transformed it, the "Kennedy-esque" scenario will not only haunt policymakers but will continue to be their excuse for empire.

WALTRAUD QUEISER MORALES

University of Central Florida

Book Reviews: International Relations

Political Change and Foreign Policies. Edited by Gavin Boyd and Gerald W. Hopple. New York: St. Martin's, 1987. 278p. $45.00.

The premise of this book is correct: the topic of foreign policy effects of political change "has been neglected in contemporary research." This is an ironic situation because politically induced foreign policy transformations repeatedly confound contemporary international politics. Leadership changes in the United States, China, and most recently the Soviet Union have in recent years altered basic cold war patterns, while regional dynamics in the Middle East, Latin American, and East Asia have been transformed by a variety of types of regime changes. Another collection of conceptual essays on political change and foreign policy, the first being K. J. Holsti's *Why Nations Realign* (1982), is welcomed as we seek to theorize systematically about the seemingly complex roots of foreign policy changes and their global implications.

Along with its ambitious theoretical introduction by Gavin Boyd, this volume is composed of three sections. The first examines changing global actors and consists of assessments of shifting political conditions and their impact on foreign relations of the United States (Callahan); the Soviet Union (Ziegler and Kanet); Britain, France, and West Germany (Papademetriou, Boyd, and Hopple); Japan (Tsurutani); and China (Boyd). The second section looks at changing issue area behavior, with chapters on political cooperation (Boyd), foreign economic policies (Doran), and international security policies (Hopple and Gathright). The final section considers system change and has chapters on the political economy of North-South relations (Moon) and ecological-political sources of change in the global system (Pirages). This diversity of essays is a positive feature. For example, several authors (Boyd, Moon, and Pirages) illustrate the utility of interactive and systemic levels of analyses in assessing the origins and implications of political changes.

This is not to slight the importance of the five country studies; these chapters are one of the book's strongest features. Each presents a solid overview of the pattern and sources of change in its respective country's foreign policy across recent decades. The essays by Callahan, Ziegler and Kanet, and Tsurutani are especially rigorous and innovative in identifying and describing distinct political periods in the foreign policies of the United States, Soviet Union, and Japan. Taken together, these essays point (as do the issue area chapters by Doran and Hopple and Gathright to the centrality of two political phenomenon: shifting opposition-factional constraints within the government and the belief systems of alternating leaders (it is somewhat puzzling that Callahan does not consider the rich literature on the belief systems of different U.S. administrations). Each chapter is well documented and includes a good selection of recent articles on the politics of foreign policy in these countries.

The major deficiency in the book is the lack of theoretical-conceptual development beyond the individual chapters. Political variables are, of course, very difficult to conceptualize in a way appropriate for making cross-national comparisons (particularly if one proposes using political culture as an integrative concept). Still, with the partial exception of the Hopple and Gathright chapter, this effort falls quite short of offering workable theoretical frameworks for conceptualizing political changes and linking them explicitly to precise dimensions of foreign policy. Part of this problem stems from the introductory theoretical chapter; however imaginative, the models as presented there seem unwieldy and rather speculative, partly because the linkages among each model's variables are not explained in the text. Compounding this, there is no concluding, integrative theoretical chapter in which the editors could have gleaned insights from the case studies about relevant domestic political dimensions and their precise effects on foreign policy. This would have pointed them to the direct foreign policy effects of leadership changes and thus to the task of conceptualizing leader's belief systems and political constraints.

Another matter (but not necessarily a negative one) is that the book's title is a bit of a misnomer. Its focus is not limited to domestic politics, but rather much useful attention is devoted to domestic and global economic and ecological agents of foreign policy change. Boyd's introduction stresses political factors as channeling more fundamental economic sources of change, while two of the issue area chapters (Boyd and Doran) are concerned mainly with political responses to economic interdependence. The China and USSR chapters stress domestic economic change as well as political trends. Moon and Pirages consider political changes only after extensively documenting global economic and ecological trends. This attempt to bridge the gap between international political economy and comparative foreign policy perspectives is welcomed, although one suspects that this ambitious expansion of the book's focus is one reason for the diminished attention to concrete theorizing about political effects on foreign policy.

In summary, this is a good book as far as it goes, and it is recommended to those interested in the roots of foreign policy transformations as well as the dynamics of change in the global system. Boyd and Hopple have succeeded in putting together a set of chapters that are substantively rich, well documented, and rather imaginative. Regrettably, though, as a collective effort it could have gone much further in advancing our conceptualizations of political phenomenon and theoretical understanding of their relationships to foreign policy changes. It lacks some of the theoretical precision of Holsti's *Why Nations Realign*, but then some readers will welcome some of the broader themes noted above. And the theorist working in this area will find that the case studies offer good material to be mined in developing testable generalizations about political change and foreign policy.

JOE D. HAGAN

University of Wyoming

Politics, Security, and Development in Small States. Edited by Colin Clarke and Tony Payne. London: Allen & Unwin, 1987. 238p. $34.95.

In Search of Security: The Third World in International Relations. By Caroline Thomas. Boulder: Lynne Rienner. 228p. $30.00 cloth, $14.95 paper.

Although the concept of security has been central to the study of international relations, it has been used mainly in analyses of relations among great and middle powers. These books apply an expansive concept of security, including not only political-military factors but also economic and cultural vulnerability, to the developing countries. Caroline Thomas focuses not simply on the political-military vulnerability of third world nations but also on economic dependency and problems of nation building. Contributors to the Clarke and Payne volume explore the security requirements of "small states," whose populations range from 80 thousand (Antigua and Barbuda) to 980 thousand (Mauritius).

The U.S. military invasion of Grenada, a tiny island state with a population of 110 thousand dramatized the security dilemma of small states in world politics. Since 1945 roughly 80 "microstates," mostly remnants of the British and French empires, emerged as a result of the United Nations-supported decolonization movement and the widespread desire for national self-determination. As Clarke and Payne observe in their concluding essay, the birth of these states suggests that "nationhood has been shown to have its own logic separate from the question of viability" (p. 225). Most small states have been condemned, like Grenada, to depend for their security on one of the superpowers or on their former colonial masters. Most have experienced severe economic problems as a result of size and neocolonial patterns of dependency. Moreover, the governments of small states have tended to dominate their societies and economies, a point first noted by Arthur Lewis and reiterated, although with qualification, by Paul Sutton.

The Clarke and Payne volume is part of a growing literature, largely of British origin, on small states. The concept of small states may be analytically distinguished from that of the third world: in general, small states enjoy relatively higher levels of economic development and tend to maintain democratic institutions. The volume is divided into three parts. Part A surveys the general political, social, and economic characteristics of small states. David Lowenthal argues that from a sociocultural perspective small states have the advantage of participatory politics. For example, while Bernard Coard was able to seize power from Maurice Bishop in a coup d'etat, it is also true that Grenada's smallness had allowed a band of angry citizens (a large percentage of St. George's population) to free Bishop from house arrest. On the other hand, Tony Payne shows that small states face serious economic constraints: lacking economies of scale, they are economically dependent and insecure. Part B provides a number of useful, albeit brief, case studies of representative small states, including Grenada, Malta, Gambia, and Cyprus. Most of the case studies document the crippling effects of economic dependency and overspecialization based on colonial farming practices and policies. Part C includes two essays that summarize the results of the various case studies and suggest policies that small states can pursue to enhance their security. Although Clark and Payne conclude that "smallness certainly intensifies, but is not a characterizing feature" (p. 227), they point to the need for a political economy of small size to formulate a more satisfactory and independent development strategy.

Caroline Thomas has written a superb textbook that will be useful in undergraduate courses in international relations. Her use of the concept of security provides a means of integrating studies of the third world within traditional international relations courses. Her definition of security is broad, encompassing economic, cultural, as well as traditional political-military concerns, and she deals with these concerns in separate chapters on the international monetary system, international trade, problems of nation building, nuclear proliferation, and food and health. The chapter on international monetary affairs provides an excellent overview of International Monetary Fund (IMF)-third world relations. Thomas provides technical details but makes the chapter accessible to undergraduates in political science.

Thomas's text concludes with a case study of the rise and fall of Jamaican prime minister Michael Manley's social democratic government, in which the IMF played a pivotal role. She shows how Jamaica's international vulnerability made it heavily dependent on the political and economic interests of the United States, encouraging Manley to establish closer ties to Cuba and the Soviet bloc. Although Thomas is generally sympathetic to Manley's attempt to steer a middle course between the superpowers and to assert Jamaican political and economic independence, she is, nevertheless, realistic concerning the limitations that international economic and political structures impose on third-world experiments with social democracy.

Despite his socialist rhetoric, Manley was, like most third-world leaders, eventually compelled to adopt what amounted to an IMF program, leading to austerity and sharply deteriorating economic conditions for most Jamaicans. Thomas concludes that Manley's electoral defeat at the hands of Edward Seaga reflected opposition to IMF-imposed austerity—which Manley administered—and not an endorsement of it. Internationally, Jamaica's overspecialization in bauxite and sugar combined with the increase of oil prices made it impossible for Manley to continue ambitious (in the context of Jamaica) social programs. Although Thomas rightly attributes Manley's demise not only to international economic vulnerability but also to his inability to educate the Jamaican masses and thereby to enlist their long-term support, it is unclear that political education alone could have overcome obstacles that arise from attempts to pursue social democracy in the context of acute economic and political dependency.

ALAN W. CAFRUNY

Hamilton College

Power, Production, and World Order. Robert W. Cox. New York: Columbia University Press, 1987. 500p. $45.00.

This is the first book of a remarkably original and ambitious four-volume study, *Power and Production,* to appear in Political Economy of International Change series under the general editorship of John Gerard Ruggie. This book outlines the conceptual framework used in all four volumes and includes a wealth of "historical and factual illustrations intended to bring the key concepts and theoretical propositions to life without attempting to give them an exhaustive demonstration" (p. ix). Its central premise is that production creates the material basis for all forms of social existence and generates the capacity to exercise power; but power in turn determines how production takes place and is organized.

The study begins by classifying the totality of contemporary world production into 12 patterns of production relations called modes of social relations of production (subsistence, peasant-lord, primitive labor market, household, self-employment, enterprise labor market, bipartist, enterprise corporatist, tripartist, state corporatist, communal, and central planning). While each mode of social relations of production "exists in relationship to other contiguous modes, . . . in a society regulated by a state, [and] may have far-reaching links in the world economy" (p. 15), at the outset Cox treats each mode as a Leibnizian monad, as self-contained structures each with its own developmental potential and distinct perspective on the world. Social relations of production, according to Cox, arise in three analytically distinct ways: (1) the accumulated social power that determines the nature of production, (2) the structure of authority as shaped by the internal dynamics of the production process, and (3) the distributive consequences of production. One of the many strengths of Cox's study is his demonstration of how these three aspects are dialectically related in a single historical whole within which contradictions frequently arise. A sense of deprivation in rewards by one group of producers, for example, "leads this group to struggle effectively for greater control of the production process, and this results over time in a change in the structure of social power" (p. 12). By virtue of the subtlety and sophistication of its analysis, Cox's study should compare favorably with other recent attempts to reconceptualize the nature of power, such as Michael Mann's *The Sources of Social Power* (1986) and John A. Hall's *Powers and Liberties* (1985).

After initially distinguishing between simple reproduction (the reconstitution in the next production cycle of the same society as produced in the first) and expanded reproduction (the generation of a surplus that enables the society to grow and change through successive cycles of reproduction), the author contends that "only in expanded production can one speak meaningfully of development, and accumulation is the means of development" (p. 398). In the remainder of part 1 Cox outlines the evolution of two basic modes of development: capitalist and redistributive. Both modes accumulate in order to grow and both may organize production in similar ways in order to generate a surplus for accumulation. But the mechanisms and underlying rationale driving the accumulation process in the two modes are different. Capitalist development is driven by opportunities for realizing profits in the market, and perceptions of these opportunities determine what is produced. In redistributive development, what is produced is determined by decision of political authorities. To a large extent, Cox argues that the initial dynamics (and repressiveness) of redistribution development were determined by the key problem such regimes were forced to confront: how to overcome the agricultural gap vis-à-vis the leading industrialized societies of Europe and North America. While redistributive development thus began with a duality of central planning and communal modes, with the "latter being placed in a planned subordinate relationship, transferring surplus to the dominant mode" (p. 100), more recent developments reveal different patterns of change in the Soviet Union and Eastern Europe, on the one hand, and China, on the other. In the former case, the communal mode has become assimilated to the requirements of central planning, complemented by a "healthy survival" of the self-employment mode, while in China, the communal mode has largely been dismantled in agricultural production, with forms of the enterprise labor market also being revived. The fourth volume of *Power, Production*, also to be written by Cox, will deal more extensively with production relations in noncapitalist modes of development.

In part 2 of the book, a wide-ranging historical and comparative survey of the development of *raison d'ètat* and the modern state system, Cox examines the all-important role of the state and the constraints imposed on its autonomy by the global political economy in explaining the transformation and contradictions of different modes of the social relations of production. Thus, although production is the point of departure in his study, Cox observes that "the crucial role, it turns out, is played by the state. States create the conditions in which particular modes of social relations *achieve dominance over coexisting* modes, they structure either purposively or by inadvertence the dominant-subordinate linkages of the accumulation process" (p. 399). Nonetheless, individual states do not pursue their goals in isolation: "Each state is constrained by its position and its relative power in the world order, which places limits on its will and its ability to change production relations" (p. 399). Throughout his analysis, Cox draws heavily on the work of Antonio Gramsci—particularly his notions of historic blocs, hegemony and counterhegemony, and Fordism—to illuminate the tasks and the internal and external constraints placed on the pursuit of state policy, as well as transformations in the structure of the world order.

The fruits such an approach might bear become especially clear in part 3, where the global economic crisis of 1973-74 is examined "to see what transformations in structures of production, states, and world order they portend" (p. 2). The industrial and financial restructuring of the last decade-and-a-half and the weakening of the United States international hegemony have led to the destruction of "labor's autonomous collective social power" and to what Cox calls the increasing "peripheralization of the labor force." Employers have been able to "shift the burden of uncertainty from themselves and the core group" of full-time career employees to various low-paid, less securely employed peripheral groups "segmented into several distinct groups having little cohesion" (p. 323). The second and third volumes of *Production and Power*, by Jeffrey Harrod, will explore the patterns of production affecting the life chances of the "unprotected" and the more "established" workers, respectively.

Production, Power, and World Order is a work of vast learning, analytic rigor, and clarity of purpose. It reflects the merits and considerable challenges of doing synthetic, integrative scholarship, in an age of disciplinary fragmentation and hyperspecialization, skillfully crossing the boundaries of political economy, international relations, industrial relations, comparative politics, and social and political thought. Readers undoubtedly will find much in the book that provokes critical reflection, whether it be Cox's clearly presented sympathy for the plight of labor and radical social change in our times (pp. 273, 349-53, 393-403), his interpretation of particular case studies and historical episodes, such as the global crisis of the early seventies, or his criticisms of other approaches and schools of thought (pp. 357, 395-96).

For all the concern with the state and its relative autonomy, I discerned a persistent ambiguity and reductionism about the nature of specifically political activity and institutions. Liberalism, we are told, had only "a circumstantial connection with political pluralism and parliamentary government in the British case. Regimes in other countries proved capable of achieving the same balance between economy and politics under authoritarian auspices. Both were 'liberal' in the sense discussed here" (pp. 127; cf. 162, 189). Perhaps those who, like Schumpeter, discerned a less circumstantial, far-deeper affinity between certain political-constitutional forms and institutions and economic development were on to a truth we forget only at the cost of our collective hardship and peril. Notwithstanding this reservation, however, *Production, Power, and World Order* and the larger study of which it is the initial volume, will surely repay our attention in ways far beyond the capacity of a brief review to convey.

DOUGLAS E. WILLIAMS

Queen's University

Reflections on the Cuban Missile Crisis. By Raymond L. Garthoff. Washington: Brookings, 1987. 159p. $18.95.

Much has been written about the Cuban missile crisis—too much, some might argue, for despite its role as a turning point in the evolution of the U.S.-Soviet global rivalry, it was a unique confrontation between the nuclear superpowers. Decision makers as well as academics must be wary of generalizations drawn from a single case. Moreover, there were many other severe and important crises since World War II, some with the U.S. and the USSR as direct adversaries, notably the crises over Berlin between 1948 and 1961 and others with more regional significance, such as the 1967 Six Day War and the 1973 Yom Kippur War in the Middle East, the 1962 Sino-Indian Border War, and the Angola crisis in 1975-76. Nevertheless, the missile crisis continues to fascinate participants, readers, and scholars alike.

Raymond Garthoff, who served at the time as special assistant for the Soviet bloc in the State Department's Office of Politico-Military Affairs, marks the twenty-fifth anniversary with a thoughful analysis of the lessons of the missile crisis. Although this slender volume contains much that is familiar to students of "the most intensive, dangerous and climactic crisis of the cold war" (p. 1), it is a valuable contribution at several levels.

First, Garthoff offers fresh insights into the *Soviet* decision process. Second, by emphasizing the "disjunction in perceptions of reality . . ., one of the critical shortcomings of the situation in 1962" (p. 128), he draws our attention to the pivotal role of the perceptual dimension in the analysis of international and foreign policy crisis. Third, he offers the perspective of a middle-level adviser to the core U.S. decision-making group; several of his recommendations, in six hitherto-unpublished memoranda written during the crisis, bear a striking resemblance to positions adopted by the president or the Ex Comm (though it would be questionable to infer that his was the decisive input at any of these decision points). Fourth, there are some minor revelations, for example, the blowing up of a Cuban industrial facility on 8 November 1962 by a U.S.-dispatched Cuban covert action sabotage team, unplanned by and unknown to the Ex Comm, even though these Mongoose operations had been suspended on 30 October! Fifth, Garthoff emphasizes that the missile crisis did not end with the Kennedy-Khrushchev exchange of letters on 26–28 October; rather, the postcrisis period from 28 October to 20 November (the "settlement") witnessed tough bargaining and continued high stress over several unresolved issues: "offensive" weapons other than the missiles, mainly the IL-28 light bombers; inspection arrangements; methods to ensure the prevention of future reintroduction of offensive weapons; U.S. assurances against an invasion of Cuba (about which Garthoff makes a dubious distinction between the president's "pledge" and the absence of a U.S. "guarantee"); and the timing of the end of the quarantine. There is valuable implied advice here for the analysis of other crises. Namely, it is not sufficient to examine the peak or most intense phase, the crisis period; the postcrisis period, too, merits attention.

Garthoff summarizes the positive short-term consequences of the crisis, the "aftermath": a turn toward detente, reflected in three arms control agreements within a year; greater resolve to agree on what became the nonproliferation and SALT agreements years later; no crisis over Berlin in more than two decades; and the absence of a U.S. threat to invade Cuba. Within the Soviet Union, too, the missile crisis led to a basic shift in Soviet policy towards the West, apart from Khrushchev's downfall in 1964.

Perhaps the most intriguing part of this book is the analysis, some of it speculative but no less useful, of the "long-term legacy": "The global strategic balance is much less important in deterring or resolving [superpower] crisis . . . because the prospect of nuclear war deters even leaders who command an overwhelming superiority" (p. 114). A specific tactical lesson was "the virtue of limited and graduated application of force" and the need of diplomacy to resolve crises.

Garthoff exudes optimism from the Soviet lessons that he discerns from an article in a confidential general staff journal of the time ("Can [the crisis] open a new and better page in the post-war history of international relations?"), as well as from Soviet political scientist–commentator Burlatsky: the need to control escalation; the value of direct, secret communication between leaders; a readiness for compromise: the grave danger of bluff, of challenging a stronger adversary, and of placing an adversary in an inextricable position during a crisis; the recognition that crisis avoidance is superior to crisis management; and the conviction that political accommodation is possible on the basis of rough equality.

These sources seem a flimsy basis for inferring such far-reaching lessons. Nonetheless, this is a perceptive and insightful contribution to our knowledge about the missile crisis.

MICHAEL BRECHER

McGill University

American Security in a Changing World: Issues and Choice. Edited by Joseph Richard Goldman. Lanham: University Press of America, 1987. 328p. $28.50 cloth, $15.75 paper.

Goldman has done us a service by his selection of these essays, which cover the central issues in the current U.S. security debate, serving scholar and student alike. There is much here to learn and to argue about; for instance, the comment by Holland and Hoover, "The myth that national security is crafted and applied in some uniquely and distinctly rational process has died" (p. 123). The variant assessments of the essential characteristics and requirements of U.S. national security, from Ravenal's appeal for strategic stability founded on the prudence and rationality of world leaders and decision makers to Sibley's argument that unilateral disarmament is a viable and appropriate road to security, offer more than adequate demonstrations that this statement is true.

There is, however, a further myth that needs to be disaffirmed, namely that U.S. security must be unalterably grounded in the perception of the constancy and severity of the Soviet conventional and nuclear threat, which leads to the common definition and analysis of security in military terms, either more or less and at what cost. Six of the 11 essays deal with military matters, for example, Cole and Lockwood on NATO and Berkowitz on defense of the North German Plain; two of the essays, one by Aldrich, Duval and Weldes, the other by Boulding, offer conflicting comparative data on the financial burden of defense. The three essays by Hopmann and Anderson, Russett, and Sibley are firmly on the arms control-disarmament side of the debate.

What is generally lacking in the selections is an adequate analysis of the relations between foreign and defense policies. However, Cole and Lockwood do deal with the differences between the United States and the NATO allies on the assessment of the Soviet threat and the "continuity of aims and plans vis-à-vis the USSR" (p. 274). But more on the differences between defense and foreign policy objectives would be helpful, for instance, on how much security is actually purchased or attained through the United States' arms exports to the third world or the Middle East. Perhaps that would be asking for too much.

Goldman states that the essays are grouped basically into the categories of political economy, national security, and international security. He is certainly correct about the first two. But I look in vain for the third, unless it is subsumed in the common U.S. view that U.S. and international security are synonymous categories. As a Canadian therefore one concerned with Canadian and global, as well as U.S. security, I find this a vainglorious assumption. Acknowledging the veracity of it, I am therefore not much assured by Ostrom and Simon's finding that "there is a strong institutionalized role which provides the President with the mechanics, rationales and discretion for using force," fostering a need for power that "creates an incentive to use military force" (pp. 195-96).

Furthermore, Goldman's selection of the title *American Security in the Changing World* offers the expectation that the reader will be told something about how U.S. security policies might or ought to be responding to the central factors of a "changing world." It is time to ask whether the essentials of U.S. and world security are immutable and forever unalterable?

As in most collections of essays covering a wide area, as in William M. Evan and Stephen Hilgartner's *The Arms Race and Nuclear War* (1987), we have, nonetheless, a rich menu of ideas and arguments. Russet follows a logic similar to Sibley in the proposition that weapons reduction could lead to lower costs while still providing a sound basis for deterrence. Perhaps such a policy could help shift budgetary expenditures from the military to social welfare. But then Aldrich, Duval and Weldes argue that these two budgetary factors are not interdependent. Or would it be better to follow Boulding out of the threat system into the cost-benefits policy of unilateral national defense? This ties in well with Hopmann and Anderson's call for an alternative nuclear strategy and a better policy of war avoidance and the management of conflict.

All these arguments are old, yet new and innovatively and cogently presented. They are essential to the comprehension of U.S. security needs and hence to the U.S. and international security debate. Why argue, why debate? Because the monetary and social costs and the ultimate risks of inappropriate policies and faulty decision making are extremely high for the entire world, as well as for the United States.

This is a valuable and recommended collection for the analyst, the critic and the student. I would hope that Goldman will venture further afield "in a changing world" in a subsequent collection of essays.

HENRY WISEMAN

University of Guelph

Breakthrough/Proriv: Emerging New Thinking: Soviet and American Scholars Issue a Challenge To Build a World beyond War. Edited by Anatoly Gromyko and Martin Hellman. New York: Walker, Moscow: Progress, 1988. 276p. $19.95 cloth, $9.95 paper.

Prospects for Peacemaking: A Citizen's Guide to Safer Nuclear Strategy. Edited by Harlan Cleveland and Lincoln P. Bloomfield. Cambridge: MIT Press, 1987. 159p. $15.00.

The 1980s may be remembered as the decade in which citizens of the planet took the first substantial steps to end the threat of our extinction in nuclear war. For the first half of the decade, the popular response primarily revolved around rediscovering the problem, learning once again just how dangerous the threat was—something we saw in the popularity of books like Jonathan Schell's *The Fate of the Earth* (1982) or the television film, *The Day After.* Gradually, however, popular attention has shifted away from concern about the magnitude of the problem toward a search for a solution, including both policies that could work and steps people could take to make them happen.

These two books are among the first serious attempts by the academic community to enter that dialogue. They are very different in style, tone, and most important of all, message.

Breakthrough has a double significance. It is the first collaborative effort by Soviet and U.S. scholars on a political subject. Begun as a project of the nonpartisan and educational Beyond War Foundation and simultaneously published in the Soviet Union and the United States, it brought together nearly 30 distinguished Soviet and U.S. scholars from the natural and social sciences. The two teams met regularly during 1986 and 1987 and agreed on the content of each of the 30 articles, which are included in identical form in the Soviet and U.S. editions.

Even more significantly, *Breakthrough* introduces a new approach to the nuclear threat to the academic community. Despite the popularity of "new thinking" in the Soviet Union and the growth of organizations like Beyond War here, political scientists for the most part continue to work on the assumption that whatever the solution is, it will have to be found working within the current scheme of international relations. The authors of *Breakthrough,* on the other hand, claim to have made a very different case: Because war is inevitable as things stand now, it can only be avoided through a dramatic change in the basic values and assumptions both sides share and that have us on a collision course.

The four sections of the book make that case and lay out the authors' alternative, beginning with a series of articles, most notably that by Paul Bracken and Boris Raushenbakh showing how the cumulative probability of computer error, misperception, uncontrollable crises, and the like make war inevitable. The second section, including a powerful article jointly written by André Melville of the Institute of USA and Canada Studies and Jerome Frank of the Johns Hopkins Medical School shows how our pathological "images of the enemy" make constructive conflict resolution all but impossible and reinforce our inevitable drift toward war.

The second half of the book is far more optimistic, outlining in articles by Richard Roney, Robert Axelrod, Anatoly Gromyko, and Andrei Kortunov what this new thinking entails, especially the proposition that the only way to achieve security in the nuclear age is through mutual cooperation. The book concludes with articles on how change can and must occur in both countries, including remarkably open and insightful pieces on *perestroika* and the prospects for reform in general by Aleksander Belchuk and Fyodor Burlatsky.

Prospects for Peacemaking is a far less ambitious but no less powerful book. Begun as a project sponsored by the University of Minnesota's Hubert H. Humphrey Institute of Public Affairs, it brought together area citizens and a group of experts. Out of that dialogue grew this collection of articles also by some of the most distinguished scholars in the field, including Marshall Shulman, John Marshall Lee, Michael Nacht, Jane M. O. Sharp, Dean Rusk, and the editors.

The book is based on a totally different assumption. As Bloomfield put it, "Strategic policy is no exception to the rule that large systems can usually be reformed only at the margins" (p. 124). That is what the book sets out to do: help the citizen see nontrivial changes that they can help produce within the context of the current system of domestic and international politics.

The articles stress two themes. First, we must rethink what has been an almost total and reflexive support for the arms control process. Nacht and Bloomfield, in particular, argue that we must understand that arms control is no panacea and that we must also develop ways to control crises and limit the likelihood of war. Second, all the authors imply that meaningful change can only occur if citizens inform themselves about many of the dangers of crisis management or Soviet-U.S. relations and then build more support for the arms control process.

Breakthrough will probably prove to be the more important book. Its message is newer. It is less technically written and, if my course is any indication, works extremely well in the classroom. It is also a publishing milestone; the first U.S. and Soviet printings (15 thousand and 30 thousand copies) both sold out before the book was officially published.

That should not take away, however, from the importance of *Prospects for Peacemaking.* It, too, is a well-written book whose message, especially about crisis management, has a huge potential audience. It, like *Breakthrough,* is a call to the academic community to come out of its ivory tower and play an active and needed role in the process of change.

We should all read them both because they both help show us the role we can play and have to play as professionals in making the decade that started with our rediscovery of the nuclear threat end with the first major steps back from the precipice.

CHARLES HAUSS

Colby College

Soviet International Law and the World Economic Order. By Kazimierz Grzybowski. Durham: Duke University Press, 1987. 226p. $42.50.

This updated and expanded version of the author's earlier work, *Soviet Public International Law and Diplomatic Practice* (1970), provides useful insight into the conceptual roots and everyday practice of international law as formulated and executed by the Soviet state. In the Leninist perspective, law is an element of the political superstructure and an instrument in the hands of the ruling class. Soviet law is an extension of Marxism-Leninism that provides identity to this communist giant and helps to shape its socioeconomic objectives. In other words, international law is an extension of politics by other means, specifically it "is the internationalization of the revolutionary struggle."

The Soviets assume that the class division of the contemporary world is also evident in the existence of separate international legal norms that regulate relations among socialist states. Moscow recognizes three systems of international law: the socialist international order, which represents a completely new and dialectically higher type of relations among states and nations; capitalist international law, which sanctions the predatory behavior and exploitation inherent in this system; and general international law, which applies to all states regardless of their domestic system. In sum, the Soviets are determined supporters of the class essence of international law and champion socialist international law as a superior and potentially universal legal order.

Understanding Soviet law requires the immanent approach adopted by the author. With such an approach it is possible to see the dialectical unity of opposites expressed, for example, in the Brezhnev doctrine of limited sovereignty and the Soviet constitutional commitment to peaceful coexistence, national self-determination, respect for human rights, and noninterference in domestic affairs of other states. Like any imperial and monolithic order, the Soviets think and act under the assumption that there is a strict hierarchy to legal systems. In addition, the ultimate law—the law of class struggle—provides the Soviet state with an obligation to execute a sui generis judicial review in case other socialist states try to impose norms of a lower legal order—the principles of sovereignty that originated under capitalism and were based on the objective laws of history. It is apparent that one of the principal functions of the Soviet international law is to legitimize Soviet politics.

Grzybowski's discussion of theory is a stepping stone to his presentation of international practices. The Soviet Union discontinued autarkic economic policies after Stalin's influence faded, and it entered international commerce and other relations with foreign nations. A more active role in international affairs compelled the Soviet Union to develop its own views, or to negotiate with mutual agreements, on many practical international issues, including the law of the sea, scientific and technological cooperation, international disputes, economic integration within the "socialist commonwealth," and so on. The author's wide-ranging survey of Soviet positions on major international issues includes historical background, discussion of Soviet negotiating positions on each aspect of international law, and an analysis of the current state of affairs. Thus, the book is a useful guide for those with an interest in international law, particularly from the Soviet perspective. The author's ability to relate theory to practice and to integrate ideology, politics, and international law, are the main assets of this study, which was founded on careful review of a large number of the original Soviet and East European sources.

The principal limitation of this book is the omission of a number of issues crucial to a comprehensive understanding of the subject. For example, the rapidly growing body of international law regulating the use of outer space and celestial bodies was not addressed, despite its enormous economic importance. Moreover, the complex questions of diplomatic immunity, responsibility of individuals, Soviet attitudes toward the International Court of Justice, and the legal norms governing the use of force, genocide, and arms control—all of great academic and practical relevance to those involved in relations with the Soviet state—were not incorporated. This selective presentation of the Soviet international law reduces the study's practical applicability.

Soviet International Law and World Economic Order is a significant contribution to the Western literature on Soviet law, but it falls short of laying out the entire spectrum of international legal institutions developed, interpreted, and practiced by Moscow and its client states, particularly these institutions' economic impact on international relations.

ARTHUR R. RACHWALD

U.S. Naval Academy

Pursuing Food Security: Strategies and Obstacles in Africa, Asia, Latin America, and the Middle East. Edited by W. Ladd Hollist and F. LaMond Tullis. Boulder: Lynne Rienner, 1987. 357p. $38.50 cloth, $16.95 paper.

Arms and Hunger. By Wily Brandt. Cambridge: MIT Press, 1987. 208p. $7.95 paper.

A couple of decades ago Lester Brown warned that with the world population growing rapidly and so much of what farmers produced being consumed by beef eaters in the industrialized nations, the world was about to run out of food. Instead, world production rose subsequently, even reaching record levels in places like India and Indonesia, giving hope that the "green revolution" was at last paying off. Soon, however, droughts struck sub-Saharan Africa, reminding us that higher production in Asia and North America was doing little to alleviate the starvation of millions elsewhere. Once more, experts and public officials faced problems that neither seemed able to solve.

In 1985 political scientists W. Ladd Hollist and F. LaMond Tullis invited some economists, political scientists, and U.S. government officials to Utah State University to share their views about this, focusing primarily on why the ambitious development strategies that were launched in the 1960s failed to achieve "food security" throughout the third world. In papers that explore causal connections between technology, economics, and politics in Africa, Latin America, Asia, and Middle East, they offer an assortment of explanations.

No such inquiry can begin, however, without clearing up the perennial issue of whether production has anything to do with alleviating the hunger problem. In chapter 2 Keith Griffin insists that it is not the issue, and advises that pretending it is distracts attention from the real causes of poverty as well as its eradication. In what is surely the most stimulating part of the volume—and the only place where a comment on a paper is offered—Alain De Janvry rebuts Griffin, warning that any flight from the production issue is just as risky, since too often it generates an antiproduction bias that impairs the pursuit of greater production, which, though not sufficient, is necessary to reduce poverty.

The contrast between Africa and the rest of the third world is stressed, though there is no consensus as to what to do about it. Cheryl Christensen offers an indictment of African governments for abusing their farmers, contending that they have failed to recover from their frequent natural disasters because of inadequate institutional support, government policies that discourage efficiency, and urban coalitions that prevent transferring wealth to the countryside. As Christensen sees it, the solution lies in the marketplace; better prices and greater reliance on demand rather than supply management, which—though unpopular in the cities—are all that stands between rural development and greater starvation. It is left to Michael Lofchie to add that as exporters of agricultural commodities, African land use is influenced as much by their reliance on income from exporting commodities as by demand for food within national markets. As a result domestic markets alone will not solve the production problem.

Chapters that deal with the various regions vary in depth and quality, though most adequately report on the prominent problems currently faced in food production and the policies that have addressed them. Regrettably, the editors fail to conclude the volume with a systematic comparison of progress and failure across regions, identifying as well as possible the most prominent causal connections among the relevant economic and political factors that have shaped national and international agriculture and prevented the reduction of poverty. Are there lessons in the Indian experience with grain or Chinese agrarian reform that Africans need to learn? And what should one conclude about the "production-hunger" issue after studying several cases: Is poverty simply a result of maldistribution, or is there enough evidence to support those who argue that most efforts by the state to achieve a more progressive redistribution inevitably impair production, creating a vicious circle from which freer markets offer the only means of escape?

Willy Brandt titles his book *Arms and Hunger,* but it shares little with the previous volume other than a desire to extricate third-world nations from poverty. He is concerned with much more than either arms or hunger. In something of an update of the Brandt Commission Report, *The Common Crisis North-South* (1980), and his subsequent *North-South: A Program for Survival* (1983), along with observations made during his recent travels in the south, he comments on everything from the world debt, UNICEF, and the environment to the Pope, democracy, and starvation. The volume is filled with truisms (e.g., on pp. 82–83, interdependency prevails in our world today; ideological differences cannot be resolved by violent conflict; our ignorance makes people slaves of their circumstances—the kind of insights that are informative only when they are read for the first time). He also exaggerates the effectiveness of recent third-world efforts to collaborate against creditors. What is different about this work, however, is Brandt's unusual frankness and pessimism.

In what he may have thought was necessary to provoke more constructive implementation of the commission's prescriptions, he complains that nearly everything has deteriorated in most third-world nations since the report's publication in 1980. Worse still, nothing is being done about it. As before, most of the blame rests with the wealthier nations that refuse to repair an international system of their own making. They remain too fixed on their own immediate problems and are imprisoned by their short-term self-interests. But he also criticizes the third-world nations, though he is careful to insist that his references to incompetence, social arrogance, and injustice within third-world governments are only meant to serve constructive purposes. No doubt many persons in the countries he examines would agree with his complaints about both worlds—privately, at least. Unfortunately, when it comes to prescriptions, Brandt resorts to familiar oratory, urging more "realism" and "cooperation." A noble and welcome gesture, but more insight into what all of this will produce is essential to make it happen.

GARY W. WYNIA

Carleton College

Sovereign Statehood: The Basis of International Society. By Alan James. London: Allen & Unwin, 1986. 288p. $37.95 cloth, $14.95 paper.

Alan James is concerned about the "intellectual quagmire" that muddles the use of the word *sovereignty.* He observes that most of the problem arises from the fact that theorizing about sovereignty has focused almost entirely on the internal nature of the state. His inquiry is directed at the role of sovereignty in international relations. Turning aside the work of "political theorists" and "historical inquiry," he focuses on state practice in the middle and later parts of this century: "What will be pursued is the question of how, nowadays, sovereign states give meaning to the word 'sovereignty' when they refer to that which makes them eligible for international life, and how this usage is to be distinguished from others of international relevance." In taking this approach, James asserts that his inquiry is about the "basis of international relations, and not its dynamics." It is basic because it deals with identification of "far and away the most important actors" (p. 8).

James criticizes "observers" for not drawing distinctions between various usages of the word *sovereignty*. Believing it to be "nonsensical to talk of states being sovereign in the sense of possessing ultimate authority . . . in relation to any of the [other states]" (p. 5), he defines sovereignty as constitutional independence: "All that constitutional independence means is that a state's constitution is not part of a larger constitutional arrangement." This attribute defines which territorial actors are "eligible to participate fully and regularly in international relations" (p. 25).

Most of this book consists of analyses of arguments that might turn aside the author's application of his definition of sovereignty to the present condition of states. Included are discussions of the impact on sovereignty of new communication technologies, nuclear weapons, economic interdependence, international governmental organizations, and supranational organizations. He agrees that all of these factors may limit political independence but concludes that they have no "bearing upon constitutional separateness, and, in consequence, none upon their sovereignty" (p. 191). At the same time he insightfully takes note of how states use international organizations to advance their purposes.

James does not avoid the difficulties presented by divided states, by the "homelands" given sovereignty by South Africa, or even by "collapsed states" such as Lebanon. In this last case he concludes that "doubts which can arise over the application of the test of sovereignty are fairly easily dispelled." This because sovereignty from an external point of view is unimpaired since other states see it as useful in preserving the international order.

This is a remarkably penetrating and exhaustive argument for constitutional independence as the basis of *interstate society*. As such, all those with a general interest in the topic will find this volume to be useful. On the other hand, in the end the analysis does not justify the subtitle's assertion that sovereign statehood is "the basis of international society." This claim rests not on analysis but on the author's affirmation in chapter 1 that states are "far and away the most important [international] actors" (p. 8). Only in the concluding chapter does James devote attention to the meaning of *international* and the meaning of *important* in this statement. He acknowledges that if "by 'international' is meant everything that goes on across state boundaries, then there is a great deal more to consider than inter-state relations." But as to which of these activities is important, his view is that this "will be determined by the view of the observer as to what is important." Since "it is all a matter of one's point of view," he sees "no possibility of progress in comparative analysis" (p. 274).

Thus James leads the reader back into an "intellectual quagmire"—this one perpetrated by that numerous band of international relations scholars who feel that they must base their research on the declared assumption that the state is the most important actor. Why? Alan James says that he is searching for "the foundation stone of the international society" (p. 279). Why? James himself offers convincing evidence that there are a complicated array of actors crossing state boundaries. It is obvious that with respect to specific issues and under particular conditions some states are very influential. On the other hand, James would not deny that the same can be said of transnational corporations, transnational religious movements, international nongovernmental organizations, cities, provinces, and nations (some aspiring to be states and others not). Some of these actors are new, but some even predate the state system itself. In the words of Octavio Paz, "Our century is a huge cauldron in which all historical eras are boiling and mingling."

Instead of attempting to overcome complexity by affirmations of faith in the importance of the state, international relations researchers must cope with the reality of a world with a number of "foundation stones." We must cease prejudging who is "important." Instead we should be asking how each actor crossing state and national boundaries affects the likelihood of violence, poverty, injustice, and ecological disaster. What are the possibilities for employing the potential of a constellation of actors in strategies that enable us to move toward a world with less violence, greater economic well-being, progress in human rights, and enhanced ecological balance? Where would efforts to overcome malaria be today had scientists persisted in the assumption that the most important actor was bad air?

CHADWICK F. ALGER

Ohio State University

The Implications of Third World Military Industrialization: Sowing the Serpents' Teeth. Edited by James Everett Katz. Lexington, MA: Lexington Books, 1986. 327p. $37.00.

It is one of the curiosities of our field that military-industrial production and arms transfers have been virtually ignored as indicators of key sociopolitical trends and neglected as a rich data base for testing hypotheses and theoretical constructs about international relations and economic development. Why remains a mystery since little happens in the international arena that is not reflected in the arms trade and production system.

James Katz attempts to remedy the situation. Not only does he set out to identify the process by which Third-World states acquire a military-industrial capability, but he also seeks to link that process analytically to development-security choices and regional-international stability.

The book is divided into three parts. Part 1 provides a useful description of the technology transfer policies of five industrialized suppliers. Part 2 analyzes the military production programs of major Third World arms producers, and part 3 contains two general essays.

The book's real strength is found in part 2, where the contributors provide a wealth of detail about the military-industrial capabilities of 11 arms producers and tackle a series of analytically challenging questions set forth by the editor: (1) How and why do Third-World countries acquire military production technology? (2) What kinds of arms production programs have been initiated with what impact on the civilian sector? (3) What are the consequences of these programs for regional security and for the future configuration of alliances and conflicts in the rest of the world?

These questions are probed to a greater or lesser extent in each chapter. Although the authors do not agree with each other on all issues, and some cases are better researched and more thoughtfully written then others, collectively these essays point to important new trends and considerable analytic consensus. For example, what motivates a country to pursue a military-industrial capability? A careful reading of the country studies suggests that although national security considerations are cited as more important for some countries than others, there is a general sense that political and economic factors are now playing a more decisive role in all Third-World procurement and production decisions.

There is also a surprising amount of tacit agreement among the contributors on the guns-versus-butter question, a particularly contentious issue during the 1960s and 1970s. In combination their descriptions suggest that the guns-versus-butter formula may oversimplify the economic effects of arms production and the policy choices available to decision makers. Carus's chapter on Israel and Moon's on South Korea, for example, indicate that there are positive and negative trade-offs that change over time. They detail the economic benefits derived initially from military industrialization in both countries—reduced foreign exchange expenditures, lowered procurement costs, and a "push" effect on the civilian industrial sector. But by the 1980s the situation had changed dramatically. Carus and Moon concur that past benefits are unlikely to continue in the future and that structural changes are underway.

The remaining chapters in part 2 support this conclusion. Market forces and continuing bottlenecks in the defense sector are compelling all Third-World arms producers to readjust their procurement goals and reassess their industrial strategies. By the 1980s the countries examined in this volume were moving toward a more pragmatic, three-pronged procurement policy: to support only those indigenous military industries that are economically viable and technologically practicable, to encourage licensed production of advanced foreign technologies when possible, and to buy from abroad items beyond the country's industrial capabilities.

Apparently Third-World leaders have also begun to appreciate the interdependence of their civil and military-industrial sectors and are now integrating them in support of larger economic objectives. Latham, for example, reports that in China slack military industrial capacity has been diverted to the production of civilian products, and significant technological transfers from defense to the civilian economy are being encouraged. The Egyptian military, according to Collins Dunn, operates factories that are now manufacturing civilian goods; and Weatherbee observes that Indonesia's leaders are demanding that up to 30% of capacity in defense plants be employed for production of nonmilitary goods. Increasing interaction between civil and military sectors appears to be a new trend in the Third World —one that may have important consequences for the speed and quality of their future development.

It would appear that the contributors to part 2 also concur that Third-World arms production will have little impact on the world's military-industrial or political structure in the foreseeable future. It is disconcerting, therefore, to read Ross's essay maintaining that the acquisition of military technology has made Third-World countries more self-reliant and given them the means to alter the North-South balance. This judgment sharply contradicts the conclusions reached in the preceding chapters (part 2) and oddly ignores these. Thomas, for example, maintains that India's policy of continued overseas procurement represents a shift from its earlier goal of self-sufficiency and implies that it "will never gain the know-how to achieve a good measure of defense self-reliance" (p. 155). Perry and Weiss point out that Brazil is still dependent on licenses, foreign support, imported engines, and avionics in its aerospace sector and that its naval sector is even more reliant on foreign sources. Katz reveals that Brazil's new AMX fighter is over 80% Italian in content. And both Carus and Moon discuss the dependence of Israel and South Korea, respectively, on U.S. equipment and markets.

In general, part 3 seems strangely disassociated from the rest of the book. What it sorely needs is a summary concluding chapter. Although Katz's essay describing the scientific and technological constraints facing Third-World producers underscores the dependency theme discussed above, it does not draw connections between the other theoretical concerns outlined in his introductory chapter and in the case studies that follow. Readers are thus left to make the connections themselves. These faults notwithstanding, Katz and his colleagues have added to our knowledge and understanding of Third-World military-industrialization and provided us with abundant material for cross-national comparisons and theoretical generalizations on other issues as well.

STEPHANIE G. NEUMAN

Columbia University

Condemned to Repetition: The United States and Nicaragua. By Robert A. Pastor. Princeton: Princeton University Press, 1987. 379p. $24.95.

Just as Secretary of State Haig claimed in 1981 that events in El Salvador (and, by extension, in Nicaragua and the region) represented a textbook case of Soviet expansionism, U.S. Central American policy has become for many the 1980s test case of the status of U.S.-Latin American relations, the Reagan Doctrine, and U.S. hemispheric policy in a post-inter-American security era. Pastor's book is yet another addition to this growing literature. A Carter administration insider (director of Latin American affairs, National Security Council, 1977–81), Pastor draws on his experience and internal administration documents to argue that in forging its policy toward Sandinista Nicaragua, the United States was responding to, and repeating its earlier policy toward Cuba. The author's stated purpose is to suggest how "future succession crises and revolutions could be managed more effectively by the United States" (p. xii).

After a brief history, which places U.S.-Nicaraguan relations into the contexts of the "ghost of Cuba past" and recurring U.S. themes of Democrats, dictators, and communists (part 1), Pastor moves into an exhaustive and insightful account of policymaking during the Carter years (eight chapters, constituting part 2 and half of part 3). Herein lies Pastor's contribution to the literature. No doubt tedious to the casual reader, these chapters provide the student-scholar rich insight into the complexities and cross currents of Nicaragua policy setting. During a conservative resurgence in Washington, the Carter administration was undertaking important foreign policy initiatives on (or facing crises with) Iran, SALT II, the Panama Canal, and China. Carter, predisposed in the case of Nicaragua to a policy of nonintervention, multilateralism, and good neighborliness, nevertheless oversaw a policy increasingly influenced by globalist (East-West) generalizations, traditional hegemonial assumptions, and poor intelligence regarding local realities. Pastor recounts events contributing to the administration's preoccupation with acquiring Somoza's cooperation, Sandinista National Liberation Front (FSLN) a political-military victory, finding a moderate "pillar of power" to compete with the Frente and preserving a reorganized Guardia in the post-Somoza era (despite regional allies' contentions that the national guard was discredited in Nicaragua). Pastor brings new details on the extent to which the U.S.-aligned, "moderate" elements in Managua lacked unity and political acumen and depended on a U.S.-imposed "solution" to the crisis and the role of such regional states as Costa Rica, Venezuela, and Panama in shaping 1979 events, as well as the extent to which National Security Advisor Brzezinski sought to "lean hard" on those states to accept the U.S. definition of the security threat posed by the FSLN.

Pastor's coverage of Reagan administration policy is sketchy and altogether too brief by comparison, given that Reagan's policy is the more crucial to an understanding of U.S.-Nicaraguan relations after 1979—something Pastor claims to provide, along with suggestions for better policy next time. Pastor provides neither new information nor fresh analyses, nor as provocative an account as other recent works on the period. He ignores, for example, an analysis of either Reagan's multifaceted program of low-intensity attrition warfare or the various foreign policy strategies, objectives, and undertakings of the Sandinistas. Pastor develops the unoriginal and overly simplistic themes of the evolution of a symbiotic relationship (and mutual obsession) between the Reagan ideologues and the FSLN hardliners and of an inflexible, nonnegotiating FSLN prone ultimately to basing policy on ideologically derived perceptions and influence-through-force strategies.

Scant attention is given to FSLN domestic political concessions (especially from 1984 to 1987) in response either to international pressures or their own assessments of the pragmatics of survival or to Nicaraguan initiatives to keep Contadora and other regional peace efforts alive (during ongoing U.S. efforts to undermine the negotiation path). Importantly, Pastor notes the policy inflexibility accruing from President Reagan's commitment to the Contras as the only policy option and Washington's squandering of opportunities for leverage and negotiation.

In part 4 Pastor discusses explanations for the repetitiveness in U.S. policy toward Latin American revolutions (U.S. political character, preferences for the political middle and gradual reform, and "pragmatism with a moral basis" are cited); reviews instances where more constructive policies could have been adopted; and proposes alternatives for more cooperative and integrative hemispheric policy. Key among these is the forging of "a New American Community that would coordinate a single approach to democracy" (p. 311).

Yet although he fits U.S. policy in the two periods neatly into seven stages, Pastor never fully enough examines his central premise that parallel conceptualizations of the Cuban and Nicaraguan cases drove Nicaragua policy. Nor does he discuss the events of the interim 20 years that makes such parallels dangerous as a basis for policy. Further, his future policy alternatives appear to give higher priority to "U.S.-centric" solutions (how the United States can more successfully influence the middle "from the margins") than to U.S. support for local initiatives. Cultural and historical differences between North and South American experience suggest that a single approach to democracy will remain too difficult a test of hemispheric cooperation.

Pastor's study might have been more useful recast and more narrowly directed as a case study of Nicaragua policy-making during Carter's presidency. Moreover, the crucial need for U.S. policy alternatives in the region has been more broadly and sensitively addressed by scholars and writers who share the author's concern.

MARY B. VANDERLAAN

Hartwick College

The United States and Puerto Rico: Decolonization Options and Prospects. By Roland I. Perusse. Lanham, MD: University Press of America, 1987. 177p. $24.50 cloth, $11.75 paper.

This book is a policy-oriented study designed to influence the ongoing debate on resolving Puerto Rico's troubled political status. Perusse's thesis is straightforward; it discards the prevailing relationship because it is colonial in nature and unsatisfactory for both Puerto Rico and the United States. Washington should end Puerto Rico's colonial status by incorporating the island as the fifty-first state of the union.

In the first of the five chapters Perusse recounts the development of the status problem. In the following three chapters he presents the opinion of different Puerto Rican political leaders about this issue, namely, proautonomy (Popular Democratic Party), prostatehood (New Progressive Party [PNP]), and proindependence (Puerto Rican Independence Party and Puerto Rican Socialist Party). In the final chapter he makes his recommendations for resolving the "status issue," recommendations that presuppose the active involvement of the U.S. government in the push towards the "decolonization" of Puerto Rico.

Gradual integration and assimilation into the U.S. economy and society have been constant themes in Puerto Rico's twentieth-century history. Yet culturally and socially Puerto Rico is a foreign country. Politically and economically linked to the U.S. but retaining a distinctness that pertains to the collective identity of the island's people, Puerto Rico's identity is not 'American." It is in relationship to this background that attempts to modify U.S. colonialism have been the overriding factor in Puerto Rican politics during this century. In the island's parlance this has been called the "status issue."

Most U.S. citizens do not know that Puerto Rico is a colony of the United States. Popular ignorance about such political facts is part and parcel of the particular variant of U.S. colonialism. A possible benefit of this book is in helping to inform people about Puerto Rico's colonial condition. But the basic premise of Perusse's book—"statehood," that is, final and complete annexation to the United States as part of a "decolonization" process in Puerto Rico—has to be contested.

The solution to Puerto Rico's status problem has been defined in terms of three options: some type of autonomy short of independence, statehood, and full independence. Theoretically, the Puerto Rican people can determine which solution they prefer. Perusse, following the prostatehood movement's ideology, claims that there are only two real options: statehood or independence. But the argument is falsely presented. If one accepts that Puerto Rico is a colony (acquired as war booty), as Perusse does, the problem would be more correctly addressed as sovereign self-determination versus complete absorption into the United States.

The failure to address the "national question" has been the Achilles' heel of prostatehood politics in Puerto Rico. The politics of the status issue is not simply a question of geographical discrimination against U.S. citizens, as Perusse would lead us to believe; it is the politics of a Latin American nation that is a colonial possession of the United States. U.S. colonialism has made it impossible, up to now, for the Puerto Rican people to achieve genuine national identity conducive to independence. Despite this, the basic features of a distinctive national political physiognomy are not only present but have been the main political obstacle for the achievement of statehood. The strength of an ethnically based nationalism, culturally stronger than its direct political expressions in the proindependence movement, has forced prostatehood supporters to argue that Puerto Rico could maintain its cultural distinctness as the fifty-first state.

The status issue debate has suffered from a political-juridical straitjacket that has prejudiced the visualization of possible ways out from Puerto Rico's political colonial dilemma. Unfortunately, Roland Perusse does not develop new ways of looking at colonialism in Puerto Rico. His analysis remains within the lawyers' realm, as has been traditionally the case in Puerto Rico. Colonialism is an event and a process that cannot be limited to its political-juridical aspects; in the long run these might be the least important factors.

Perusse's book must be set in its proper context. This is not an academic book but a polemic in the PNP's attempt to achieve statehood for Puerto Rico. Perusse's book should also be set in the context of U.S. cold war politics. The communist threat strongly informs his analysis as evidenced by his claims that Cuba and the Soviet Union are menacing Puerto Rico. For Perusse, ultimately, the resolution of Puerto Rico's status problem is critical because the island is an important U.S. military bastion in the Caribbean, a condition that would be affected if Puerto Rico is allowed to drift towards independence.

Notwithstanding Perusse's blatant preference for statehood, I would recommend the book to anyone interested in contemporary colonialism in Puerto Rico and the contradictory ideologies it generates.

JUAN MANUEL CARRION

University of Puerto Rico

Friends, Foes, Values, and Fears: Inquiry into Mechanisms of Thought in Peace and Security. Edited by Henk-Jan Rebel and Leon Wecke. Amsterdam: Jan Mets, 1987. 293 p. $20.00 paper.

Friends, Foes, Values, and Fears is the product of the 1986 annual meeting of the International Society for Political Psychology. Unlike many proceedings, however, this workshop effort produced a unified and integrated volume, one that is consistent with recently renewed interest in mass and elite opinion on foreign policy issues. *Friends, Foes, Values, and Fears* attempts to construct a decision-making model based on tenets drawn from the psychological and sociological traditions and then apply it in studies of public opinion on a variety of foreign policy issues (e.g., images of the United States and USSR; attitudes toward NATO and nuclear weapons). Striving for what the authors call a "personological" model of public opinion, the book attempts to shift the focus of the endeavor from aggregate opinions to the "psycho-logic of the individuals who collectively constitute the public" (p. 14). Although the authors ultimately fail to construct a model that can predict behavior or change, their summary and original application of a diverse body of literature is useful reading for those working in political psychology, political behavior, and foreign policy. Their use of diverse methodologies (Rokeach's value survey; Osgood's evaluative assertion analysis; cluster analysis; LISREL) also constitutes a comparative methodologies reader useful to illustrate the many options available in empirical analysis, with an excellent appendix discussing the project's sampling techniques, operationalizations, scalability, item analysis of Likert-type scales, and scoring procedures.

The foundation of *Friends, Foes, Values, and Fears* is its critique of public opinion research, that is, the lack of explanatory power in both the causal approaches (social origins) and the functional approaches (inner cohesion) to public opinion. To address this weakness the authors turn to social psychology and provide readers with an excellent summary of the thrust and "tension between psychological and sociological tradition" (p. 23). Drawing on the insights of Rokeach, Maslow, Adorno, Festinger, Bem, and Allport, the authors produce a "system model" that includes personality variables, values, attitudes, perception, definition of the situation, and intentions. The subsequent chapters then test various aspects of the model using contrasting methodologies to determine predictive power. Unfortunately, the authors do not integrate the work of Michael Brecher or Margaret G. Hermann, or Margaret and Harold Sprout's *The Ecological Perspective on Human Affairs* (1965) into their project. Had they done so, they might well have questioned their intent to build a model that "unites all the possible influences exerted on him/her—at least theoretically during his/her lifetime" (p. 26). Such all-inclusive conceptual frameworks have proven consistently to have poor explanatory power and tremendous appetites for data. Even the diverse empirical methods applied to grassroots opinion in *Friend, Foes, Values, and Fears* fail to make the cumbersome model work, that is, to predict behavior or change. The authors reluctantly conclude that they have found no general answer to the question, "Which enemy images are politically relevant? . . . It depends on the nature and circumstances of the political system involved, or aspects of these in which other factors, like political culture and national character, may also play a role" (p. 195). Despite the authors' failure to produce a breakthrough model, their work is strong in its innovative approach to the "black box" of public opinion, and brings fresh new ideas to the use of survey data. One can easily overlook the occasional awkward translation (Dutch to English) in this cross-disciplinary effort to conceptualize the individual in public opinion research.

DONNA M. SCHLAGHECK

Wright State University

The Carter Administration's Quest for Global Community: Beliefs and Their Impact on Behavior. By Jerel A. Rosati. Columbia: University of South Carolina Press, 1987. 259p. $24.95.

Although it is difficult to understand the present lack of scholarly interest in the foreign policy of the Carter administration, Rosati's book is a welcome addition to the sparse literature. The questions this study attempts to answer are

(1) Did the Carter administration have a worldview during its term of office? (2) Did the chief foreign policy decision makers agree on this worldview and, to the extent that they did not, how, when, and why did their views diverge and with what results? (3) Did the administration's worldview affect its foreign policy behavior?

Rosati's answers to these questions run counter to many of the textbook evaluations of the Carter administration's foreign policy. According to Rosati the administration had a definite worldview in 1977 that centered on the U.S. role in building a global community and downgraded military security-containment issues. In 1978 and 1979, President Carter and National Security Advisor Brzezinski grew increasingly suspicious of the Soviet Union's motives and behavior. They began to view security issues and containment as relatively more important than before while correspondingly giving less emphasis to many of the issues related to building a global community. During 1978-79 Secretary of State Vance did not significantly modify his initial worldview, as Carter and Brzezinski were doing, with a consequent division at the top of the administration. In 1980, after Vance had resigned, a new coherent worldview was developed but this time it emphasized national security-containment issues. Furthermore, Rosati finds that the Carter administration's foreign policy behavior followed its worldview to the extent that there was a consensus of viewpoints among its chief decision makers. Finally, the study convincingly shows that at least in 1977 and 1978 the Carter administration held a worldview distinguished from those of its predecessors by its relatively optimistic assessment of the Soviet Union and a genuine lack of interest in containment. In short, not all administrations since the enunciation of the Truman Doctrine in 1947 have made containment of the USSR the cornerstone of their foreign policy.

Although students of recent U.S. foreign policy will be interested in Rosati's findings, his study will not end debate about the questions it examines. At a minimum, there is likely to be some criticism of Rosati's theoretical foundations, methodology, and data. For one thing, the author bases a good deal of his work on the social science literature that deals with collective attitudes and the relationship between attitudes and behavior, but, as the author candidly admits, both areas are weakly developed. Even though Rosati does about as well as he can with his spotty and sometimes contradictory theoretical material, this book will not satisfy readers who require empirical studies to have a strong theoretical foundation.

Second, the content analysis of the major speeches of top administration officials (for example, Carter, Vance, Brzezinski, Muskie) as well as the development and use of an event data set based on *Deadline Data on World Affairs* will undoubtedly be met by a variety of standard objections. However, the author appears to understand most of the difficulties with his methodology and data and handles them in a competent manner. In other words, the study has its problems, but they do not disable it. And, it should be emphasized, the nonquantitative reader will almost certainly not care about these issues and will happily ignore the discussions of theory and method.

My major regret about the volume is that it made little or no attempt to deal with a number of important questions. For example, for all of its extensive discussion of the Carter administration's worldview of 1977-78, the study offers no thoughtful evaluation of its strengths and weaknesses—whether it was realistic, able to provide clear priorities among feasible objectives, or sustainable in terms of U.S. domestic support. Apart from a few scattered remarks, the author never analyzes the 1977–78 worldview, although Rosati *appears* to favor it. Despite the book's solid quality, I was left looking for more than it provided.

The book's subject matter is important, its treatment of the material is basically competent, and its findings are worth thinking about. It will undoubtedly be widely cited in future studies of the Carter administration's foreign policy.

JOSEPH M. SCOLNICK, JR.

University of Virginia

Conflict Management and Problem Solving: Interpersonal to International Applications. Edited by Dennis J. D. Sandole and Ingrid Sandole-Staroste. New York: New York University Press, 1987. 321p. $40.00.

This wide-ranging anthology on conflict management stems from a 1982 lecture series at George Mason University's Center for Conflict Resolution. Despite limitations inherent in its origins, the volume has strengths that make it a valuable resource, especially for the nonspecialist.

Its unusual organization features 14 chapters, each of which opens with a revised version of an oral presentation. Responses by two or more discussants follow each essay. The resulting volume is comprised of 42 presentations by 34 different authors. With the principal presentations averaging about 10 pages in length and the reactions varying from 1 to 8 pages, the chapters cannot provide the depth usually found in journal articles or book chapters. The variety of topics and the diversity of orientations also prohibit the chapters from building very effectively on one another. Still, the statements of conflict management theory and technique in parts 1 and 4 frequently find expression in applications to issues at the interpersonal level (in part 2) and the international level (in part 3). Thus, despite a rather odd format, the absence of an explicit orienting focus, and treatments that occasionally border on the superficial, the volume does succeed in elaborating several integrating themes of conflict management and demonstrating their utility in quite different settings.

While some of these will be very familiar to specialists, it is here that the format conspires to bring a liveliness of exposition that will both invigorate a refresher course and stimulate a first exposure. We are reminded, for example, of the generality of the concepts of game theory by the emphasis on techniques for achieving win-win outcomes and the centrality of perspectives that emphasize the positive-sum character of negotiations. Moreover, the formal concepts are given life by their application to diverse problems in a variety of contexts.

The diversity in the audiences targeted by the various authors guarantees something of benefit for almost everyone. Several of the discussions of interpersonal conflict management contain little of general interest, but the reporting of personal experiences by conflict management professionals will be valuable reading for students interested in career prospects. The discussions of East-West negotiations, by contrast, may be useful to the general reader but are too superficial to interest most scholars of international relations.

To be sure, the essays exhibit uneven quality. The most challenging essays are those of John Burton and Michael Banks, both of which approach conflict management from a broader set of concerns. Burton offers some intriguing suggestions for building an appropriate framework for conflict management, including the methodological component (he invites a reexamination of Peirce's discussion of hypothetical inference), the theoretical component (control theory that considers both "universal needs" and "bonding"), and applications (interactive decision-making and problem-solving approaches rather than reactive decision-making and negotiations).

Banks contends that students of conflict resolution are unlikely to contribute much to the achievement of peace unless they move beyond the three traditional conceptions of peace, equating peace, respectively, with harmony, order, and justice. Instead of thinking about peace as a fixed condition, they need to accept a more sophisticated conception in which peace is "a network of relationships full of energy and conflict which is nevertheless kept under social control."

As an introduction to conflict management theory, the contributions of James Laue, Morton Deutsch, Dean Pruitt, and Thomas Colosi in part 1 are especially valuable. Laue introduces a basic vocabulary that is helpful for the remainder of the volume, especially with respect to third-party intervention. Deutsch lays the groundwork for the subsequent discussions of successful conflict management by identifying a number of conflict pathologies and social-psychological processes that must be avoided. His game-theoretic constructions, which range from the classical prisoner's dilemma to more original and informal examples, are used to sketch the types of outcomes capable of avoiding these pitfalls. Pruitt's stimulating piece is the most explicit in laying out the creative approaches to negotiation that are found in varying forms throughout the volume. His discussion of integrative agreements and win-win (i.e., problem solving) approaches to negotiations serve as a theme for much of what follows. Colosi's penetrating explication of strategy and technique are very helpful in linking the theory to the practical problems of the various parties to negotiations.

Though the presentations are by now more than six years old, neither the individual contributions nor the volume as a whole appears dated. The topics themselves remain current—even if some of the references have been obscured by the passage of events from headlines—and most of the authors have done a good job of updating references through the middle of the 1980s. Moreover, Sandole's bibliographic epilogue is a useful guide to some of the more significant recent contributions to this literature.

BRUCE E. MOON

Lehigh University

National Security and United States Policy toward Latin America. By Lars Schoultz. Princeton: Princeton University Press, 1987. 377p. $42.50 cloth, $12.95 paper.

According to the dust jacket and several statements in the text (e.g., pp. xv, 13, 126), this book is a study of U.S. policymakers' beliefs about U.S. security interests in Latin America. In reality it is mostly an exposition of Schoultz's own belief that U.S. policies toward Latin America are repressive, unjust, unnecessary, unfeasible, and unwise.

Regarding the first topic, Schoultz's thesis is that "the battles that have been occurring in Washington over United States policy toward Latin America are the product of policy makers' differing beliefs about the causes and consequences of instability" (p. 308). He defines policymakers broadly to include not only executive officials but also legislators, legislative staff, former officials, and interest groups. His definition of instability is "focused on violence" (p. 37n); for Schoultz, instability does not refer merely to frequent changes in government, coups, strikes, and so on but rather more profound, "revolutionary," "chaotic" changes (p. 21).

Using these definitions, Schoultz argues that U.S. policymakers' beliefs about Latin American instability fall into three categories: that communism causes instability, which is bad for U.S. interests; that poverty causes instability, which is bad for U.S. interests; and that poverty causes instability, which is good for Latin America and for U.S. interests both. Schoultz himself believes that views 1 and 2 are wrong and have both been "deserted" by the academic community, whereas the third view has the support of "scholars who have won hands down the intellectual debate over the causes and, to a lesser extent, the consequences of instability in Latin America" (pp. 22-23). In his treatment of policymakers' beliefs, however, Schoultz finds that the first two views are dominant and that only a fairly small, though significant and growing, minority of policymakers is in the third category. The disparity between his own views and those of policymakers (as he describes them) gives rise to the criticisms of U.S. policies that pervade the book.

The data for the study of policymakers' beliefs come from the published record and interviews. The dust jacket claims that the entire book is "based" on 290 interviews, but this is inaccurate on several counts. Schoultz himself states that the 290 figure "hides so much diversity in interviewing form and substance as to be almost meaningless" (p. xii). There is a useful diagram (p. 312) from which rough proportions can be inferred but no tables or numbers stating precisely how the beliefs of policymakers are distributed.

Schoultz elaborates his own views in a number of ways. After an introduction and a chapter ostensibly on Latin American instability but really about Central America in the very recent past, Schoultz has chapters on poverty and communism as causes of instability. In these chapters Schoultz's own ideas, social science theories, and the views of policymakers are mixed together in a confused, hurried, unsystematic fashion. The overall result is superficial and polemical.

Chapters 4–6 deal with the consequences of Latin American instability for U.S. access to raw materials, military bases, military support, and maritime routes ("sea lines of communication"). Chapter 7 assesses the implications of instability for possible Soviet access to military bases. In these chapters, Schoultz argues that U.S. strategic interests are not threatened by Latin America instability. He has interesting things to say about military, technical, and tactical issues; but he virtually ignores the equally important political, diplomatic, and psychological aspects of those interests. Nevertheless, this is the strongest, most coherent part of the book. The final two chapters—on Latin America, the global balance of power, and U.S. policies—return to the loose arguments, simplistic prescriptions, and polemical style of the early chapters.

A pervasive feature of the book is oversimplification. Schoultz collapses all U.S. policy toward Latin America into security policy; all security policy into policy regarding instability; and all policies regarding instability into his three-part typology (communism, poverty, revolution). By depicting the entire region as Central America writ large, he violates the first rule of the Latin Americanist: always remember that the countries in the region are different.

Schoultz believes that the only security problems the United States faces in Latin America are those it makes for itself. The conflicts of interest that exist between the United States and Latin America when the communism and poverty approaches are used would disappear, he argues, if the revolutionary approach were used instead. In other words, there is a road to complete inter-American harmony, and it is called revolution. Schoultz rests this claim on recent social science writings, but they cannot bear the weight. Indeed, social science can never solve policy problems so easily or completely as Schoultz supposes.

Schoultz also claims to know the interests of Latin Americans. His theme is that for Latin America, the United States has long been an evil empire and remains the focus of evil in the modern world. Anything that reduces U.S. influence is good. Without the United States to repress them, he says, genuine revolutions will occur widely and bring only good things. However, Schoultz uses Latin American sources rarely and selectively. He ignores vital differences in Latin American situations and scarcely mentions recent trends toward democracy. For him, the only distinction that matters is revolution-nonrevolution.

Schoultz admits he is polemical ("judgmental"), but claims that he tried "to treat all but the most idiotic opposing positions with respect" (p. xv). But his definition of idiotic is too broad for scholarly purposes when he says that conservatives "interpret every campesino's belch as a threatening shift in the global balance of power" (p. 275) and that liberals "shot everything that moved" among the poor in Latin America (p. 309).

Although Schoultz does not relate his study of policymakers' beliefs to previous studies, there are some interesting connections. Fifteen years ago, U.S. policies toward Third-World political development, reflecting Hartzian liberal premises, clustered around what were then called cold war, economic, and explicit democratic doctrines. Schoultz's communism and poverty categories are similar to the cold war and economic doctrines. This suggests interesting lines of continuity over time. Schoultz does not report any explicit concern with democracy among policymakers. Perhaps it is no longer there; perhaps Schoultz's conceptual lenses keep him from seeing it. Finally, there was certainly no significant U.S. government doctrine favoring Latin American revolutions in the period from 1947 to 1973. If there is now, that is an important change.

The book also reflects significant changes in the beliefs of U.S. social scientists concerned with developing countries in general and Latin America in particular. Twenty-five years ago, most such scholars were incrementalist reformers; very few favored violent, profound revolution. They preferred democracies to military dictatorships. And they regarded the United States as a powerful force for good in the world. Today, most U.S. scholars of Latin

American development reject incrementalism, favor revolutions, and regard democracy under capitalism as merely "formal." They still see the United States as vastly more powerful than it really is, but for evil, not for good. The earlier visions were flawed, certainly, but are the recent ones better? Are they as good? Have we improved or merely changed?

ROBERT A. PACKENHAM

Stanford University

British Foreign Policy: Tradition, Change, and Transformation. Edited by Michael Smith, Steve Smith, and Brian White. London: Unwin Hyman, 1987. 287p. $49.95 cloth, $16.95 paper.

This book is a careful study of British foreign policy in the age of interdependence, new cold war, and Margaret Thatcher. As such, it represents the first attempt in a number of years to tackle this subject in a book-length project. The authors' purpose is not to take up the story of British foreign policy where F. S. Northedge and Joseph Frankel left off in the early 1970s but to identify "broad patterns of continuity and change" (p. xiv) and their implications for policy. The main argument, however, is that the continuing process of change in Britain's external environment in the direction of higher levels of economic interdependence and policy integration has not been matched by appropriate changes in the way the British think about or conduct their external relations. Indeed, the historical collective memory of Britain's status as a great power has resulted not only in inappropriate policies for the contemporary international system but in policies—such as the attempts to preserve sterling's role as a reserve currency or, more recently, its "independence"—that are positively harmful. This is not a novel thesis for U.S. readers nor for many Britons either. What is both novel and indicative of the extent of the difficulties that Britain now confronts is that this work has clearly not been written for "critical thinkers" but for the foreign policy establishment both in government and in the universities.

The book is divided into three sections: "Settings," "Themes," and "Arenas." In the first the Smiths identify traditionalist and transformationist perspectives on international relations. They and their colleagues subscribe to the latter approach, stressing the impact of complex interdependence on relations between states. However, they argue that the theory and practice of British foreign policy has been dominated by traditionalists or realists. According to this view, Britain has declined in a world of power politics, the essentials of which have remained unchanged. Practice, therefore, has been directed at slowing this decline and trying to substitute influence and skill for the shrinking power base. Subsequent chapters explore the historical, social, and institutional factors that may account for this and their erosion by the complex, multilateral, and quickly changing character of contemporary international engagement.

The second section deals with security defined very broadly in economic, social, and cultural, as well as military, terms. McGrew argues that economic circumstances and the fact that Britain's nuclear weapons system is worn out pose difficult choices for Britain in a context where the postwar foreign policy consensus is breaking down. Tooze suggests that the concept of economic security is barely grasped by governments that open up a weak economy to ever-greater levels of external penetration, lessen their control of it, and impose a heavy defense burden on it all at the same time.

The final section offers chapters on Britain's declining but still significant contributions to East-West and North-South relations but, more importantly, stresses the inconsistent and inappropriate character of much of Britain's policy towards Western Europe and its relations with other advanced industrial countries. Significantly, and correctly, the relationship with the United States does not get a chapter to itself.

This is an excellent and important work, both for the questions it raises and the manner in which it does so. U.S. readers may find its style repetitious, and the book is replete with English qualifiers. No trend is noted without its countertendencies. No bold statement is made without the accompanying confession that it oversimplifies the complex. But this is a book that aspires to wean practitioners of British foreign policy from their pragmatism and, by Richard Little's final chapter, to confront them with the importance not just of theory but of hermeneutic theory. The great mystery, of course, is why the British stay wedded to the categories of the past. In *British Foreign Policy* the resilience of traditionalism is noted but not really explained. This is partly a function of the reluctance of the authors to unpack the concept of interdependence. The latter is presented as an unavoidable and neutral generality to which the British must adjust. Only Michael Smith, in his final chapter, explicitly sketches out what a poor, particular deal it seems to offer Britain and the role of the state in accepting this deal. If this is so, perhaps traditionalism and the foreign policy it produces should not be regarded as the baggage of the past so much as an ideological and necessary response by the state to the threats posed for its internal legitimacy by the transformation of Britain's external and domestic circumstances and the role of the state in that continuing process.

PAUL SHARP

University of Minnesota, Duluth

Banker to the Third World: U.S. Portfolio Investment in Latin America, 1900–1986. By Barbara Stallings. Berkeley: University of California Press, 1987. 450p. $45.00 cloth, $12.95 paper.

More than any other author, Barbara Stallings provides the rare data, historical perspective, and sober reflection to understand the recurring international debt crises in the twentieth century. She compiles heretofore inaccessible quantitative series on U.S. investments in Latin America and on Latin American interest rates, premiums, budgets, trade, current accounts, and national production. That thorough exploration of the record allows her to reach cautious and convincing judgments about trends, causes, and consequences, for lenders as well as borrowers. Moreover, those complex patterns and findings are presented in crystalline prose easily accessible to the noneconomist.

Stallings establishes and analyzes different types of debt cycles by time periods. She contrasts the British experiences and theoretical explanations in the nineteenth century with U.S. lending in the twentieth century. Thereafter, most of the book is devoted to a comparison between the two major bursts of private U.S. financing in Latin America in the 1920s and the 1970s. During other decades direct investments or government loan flows loomed larger. The author fleshes out her generalizations with a splendid case study of U.S. bankers in Peru and then concludes with some savvy forecasts for the lingering crisis of the 1980s.

Stallings draws some important contrasts with the 1920s. The 1970s witnessed much more direct lending and loan pushing by U.S. banks. Whereas demand from Latin American governments exceeded supply in the twenties, the reverse was true in the seventies. In that latter period, competition among bankers to place those loans became even more feverish, rendering the predictable collapse even more catastrophic.

Stallings explains those loan cycles through an innovative model of cumulative causation, linking supply and demand factors. Necessary conditions for massive private lending included favorable but not intrusive policies by the U.S. government, conducive institutional and legal arrangements such as the U.S. Federal Reserve and the Eurocurrency market, and accumulations of income by high, rather than low, savers. Under such circumstances, relatively low rates of growth or return in the banks' home country triggered capital exports. Rising competition and expectations elevated that outflow to dangerous levels. On the demand side, Latin America required foreign financing to compensate for its weak capital goods industries, its immature capital markets, and its trade and budget deficits. High interest rates and government activism in the recipient countries helped open the floodgates for finance capital, especially when demand was slack in less-risky advanced industrial nations.

Although very insightful about international political economy, this monograph says little about the straight "politics" of international lending. Stallings does not focus on decision making by governmental and nongovernmental actors, either within or between the United States and Latin America. As a result the author slightly underestimates the role of the U.S. government in these lending bonanzas. She is absolutely correct that in both the 1920s and 1970s bankers made these loans out of profit motives, not as agents of U.S. foreign policy. She is also right that the U.S. government mainly facilitated the "dance of the millions" by not interfering or by not offering more attractive public credits. In many cases, however, that very passivity was a conscious policy choice.

At least in the 1920s, the State Department actively believed that its larger foreign policy objectives would be advanced most efficiently by encouraging a laissez-faire "open door" for the "diplomacy of the dollar." Private sector investments would on their own inexorably pull Latin America into the U.S. orbit. Policymakers realized that more overt government intervention would have been counterproductive, especially in the wake of U.S. military misadventures abroad. But behind the scenes, U.S. diplomats quietly nudged lenders and borrowers to get together and to obey the rules of the game.

Although Stallings may somewhat understate the stake and strategy of the U.S. government in the private lending binges of the twenties and seventies, she argues persuasively that its future participation should be greater. If it is, she predicts that the widespread defaults of the 1930s are not likely to be repeated in the 1980s. The main reasons are the involvement of industrial governments and multilateral agencies, the direct indebtedness of the U.S. banks (instead of scattered bondholders), and the greater interdependence of the world economy.

Stallings calls for increased engagement by public institutions not only to avert default today but also to avoid repetitive boom-and-bust cycles in the future. Her historical evidence is certainly compelling that—as the bereft bondholders complained in the 1930s—the U.S. government has not done enough. Policymakers as well as political scientists, economists, and historians will benefit from considering the weighty, prudent arguments in this monumental study.

PAUL W. DRAKE

University of California, San Diego

Arms and Disarmanent: SIPRI Findings. Edited by Marek Thee. Oxford: Oxford University Press, 1986. 478p. $37.00.

If one is looking for an "encyclopedia" of current peace research on topics dealing with arms issues and international relations, this study by the Stockholm International Peace Research Institute (SIPRI) and Marek Thee is it. In the introduction Frank Blackaby explains that the purpose of peace research at SIPRI is to address the critical linkage between the militarization of politics and economics with human survival. Most of the research in the phenomena of war and international politics, the arms race in its conventional and nuclear dimensions, the "political economy" of weapons and technology transfers, and so on, is essentially interested in posing empirical questions to find answers about these phenomena. SIPRI specifically sponsors this conduct of inquiry, to be sure, ostensibly to delve deeply into these problems in hopes of educating a much broader public than the academic on how to understand what really is at stake: the human race and its continuance on this planet.

The book is divided into several sections that will interest specialists and generalists alike. "The Quantitative and Qualitative Dimensions of the Arms Race" focuses on the methodological problems associated with data gathering. Reportage by Third-World governments is complicated by political interests (e.g., national security) as well as inability to compile or store sensitive information. Data insufficiencies also contribute to distortions in interpreting war-fighting strategies, understanding how economic aid is distributed across domestic sectors of a national economy, or evaluating Soviet and U.S. investments at home and abroad among client states. Nevertheless, the questions and models presented in this section represent dynamic research that enriches our insights into what needs to be known rather than debated.

In "Nuclear Armaments" the contributors concentrate on weapons technology and what the nuclear superpowers now have in their stockpiles. The other sections deal with space-based weapon systems and the modern battlefield for those who are inclined towards operational analysis. The rest of this collection contains essays ranging from strategic doctrine to the war in Afghanistan, and all are well researched and stimulating.

This study is more an update and a survey of recent empirical research; however, it could use a synthesis at the end to pull together what were essential questions and findings. Again, for the specialist and the generalist SIPRI offers an informative grouping of studies in relation to peace theory, and for that reason alone it should be consulted.

JOSEPH RICHARD GOLDMAN

University of Minnesota

Latin American Debt and the Adjustment Crisis. Edited by Rosemary Thorp and Laurence Whitehead. Pittsburgh: University of Pittsburgh Press, 1987. 359p. $42.95.

This is the third in a series of collections edited by Rosemary Thorp (one in collaboration with Laurence Whitehead) dealing with the economic policies of Latin American nations in the context of crisis in the international economy. The first, *Inflation and Stabilization in Latin America* (1979), focused on the impact during the 1970s of the shock of the first petroleum price increase. The second, *Latin America in the 1930s: The Role of the Periphery in the World Crisis* (1984), sought through deeper understanding of that period to discover parallels and gain insights into the contemporary situation. These are collaborative efforts, in which the participating authors meet in several seminars to compare research and approaches. The resulting volumes constitute not simply a set of essays on related topics but rather a joint effort to understand the major economic issues of the contemporary world. This volume is a significant addition to the literature.

The book deals with the results of the "shock" caused by the refusal of international lenders to make further funds available to the region, following the Mexican moratorium of August 1982, through analysis of recent adjustment policies evaluated in terms of their short- and long-term effects, emphasizing the impact on economic development. Although many national policy shortcomings are noted, the current crisis and its prolongation are clearly laid at the door of the international banking community.

The country studies are detailed chronologies of policies and their outcomes. They reveal, particularly for the more advanced economies, the wide array of options that have become available to authorities practicing interventionist national economic management. That such endeavors are not always salutary is illustrated by the Mexican example of depreciation incentives for private investment, which led to a 33.4% increase in the purchase of automobiles by private firms.

Of course, in addition to theoretical validity, these efforts require accurate measurements of appropriate economic variables. In this regard, the authors are particularly critical of the statistics for public sector fiscal deficits that fail to take into account the impact of inflation. When regional deficits are not corrected for this factor it "leads to misdirected policy goals, and thus unnecessary deflation, loss of output and employment" (p. 340). This is particularly critical for International Monetary Fund–directed adjustment programs, which mandate reduction of public deficits. The authors and editors are, in fact, generally dubious about the causal role of fiscal deficits in relation to balance-of-payments problems or inflation and regard policies based on this assumption to have been harmful. Another stricture of IMF plans that the authors see as having had negative impacts on the region are prohibitions against currency controls and import restrictions. The IMF is also faulted for having required curtailment of imports that impinged on negatively intraregional trade.

For having dealt with adjustment in a manner least deleterious to long-run development, Brazil gets the highest marks. Even during periods of retrenchment, authorities protected the long-term investments destined to have a positive effect upon the balance of payments. Brazil also avoided capital flight, a major problem for other countries, through high domestic interest rates and exchange rate–indexed government bonds, as well as exchange controls. Interestingly, the nation that seems historically to have managed its adjustment process most effectively is Colombia. The chapter on this nation, however, is flawed by the failure to include in the analysis any consideration of the impact upon the national economy of drug revenues, which have come to overshadow traditional coffee exports in magnitude.

The inclusion of a chapter on Central America is instructive, illustrating that the incentive for adopting IMF-imposed conditionality—the hope for loans from private banks on the basis of the IMF "seal of approval"—is not operative for smaller nations. Large debtors like Brazil and Mexico were the new loan and rescheduling beneficiaries because of the destabilization threat they presented to the entire international monetary system. As one contributor, the late Carlos Diaz Alejandro, has pointed out ("Latin American Debt," *Economic Activity* 2:1984), this involved coordinated actions by the IMF, developed nations' financial agencies, and large private banks, equivalent to a creditors cartel, and was accompanied by more stringent terms and insistence on public assumption of responsibility for the repayment of privately incurred debts (a matter that the country studies fail to document).

The approach adopted by the authors in this work takes into account political factors beyond the narrow technical considerations used by monetarists and even some structuralists. However, although the distributive implications are discussed in the conclusions, they are not systematically addressed in the country studies. Also, one wishes that the authors might have addressed the point raised by Charles Kindleberger in the collection of the 1930s, that reduction in prices to increase exports for balance-of-payments effects has a damaging impact on production for the internal market. More attention to the effects of foreign direct investment, especially in assembly industries, would also have been instructive.

The editors conclude that amelioration of the Latin American economic situation was unlikely to be forthcoming on the basis of near-term changes in the international economy. International capital was absorbed by the U.S. need to finance its own budget and trade deficits. By the middle of 1985 lending to Latin America "had ceased to grow, even in nominal terms" (p. 337). They considered possible measures of regional self-help: diversification of exports, increased trade within the region, the luring home of flight capital, and cooperation in negotiations with lenders, including the threat of a moratorium. These were judged to offer moderate potential for improvement, although they recommended "internal restructuring" for balance in external accounts as more likely to secure an improved relationship with creditors than threats. This they conceded would be more of an option for Brazil than Peru or the nations of Central America.

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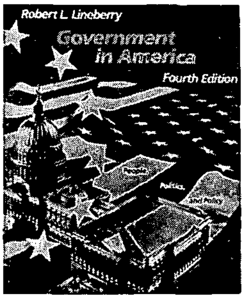
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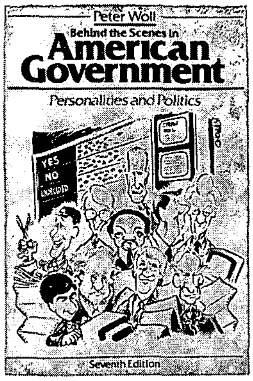
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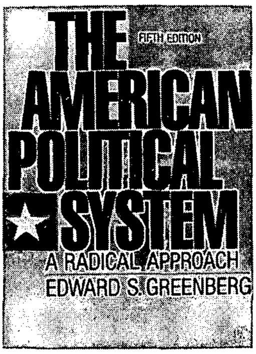
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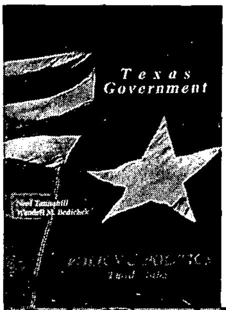
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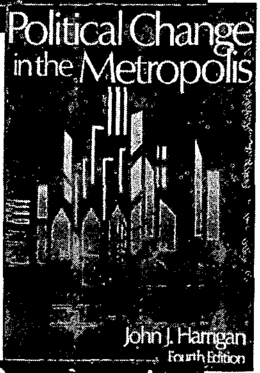
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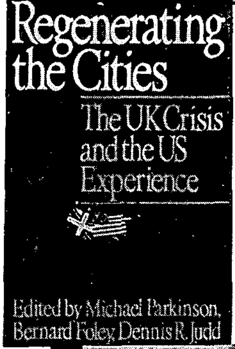
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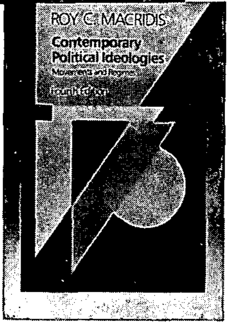
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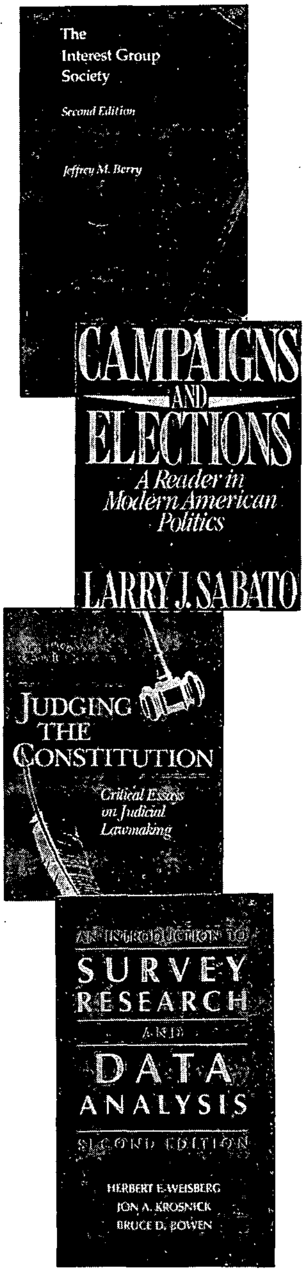
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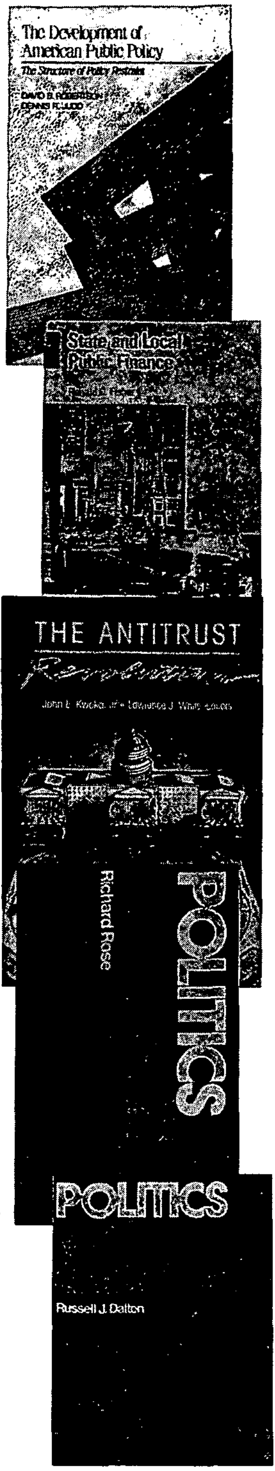
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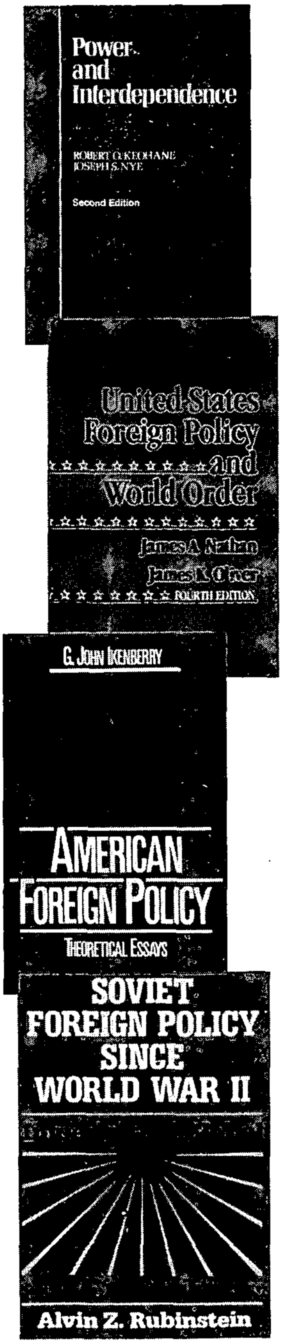
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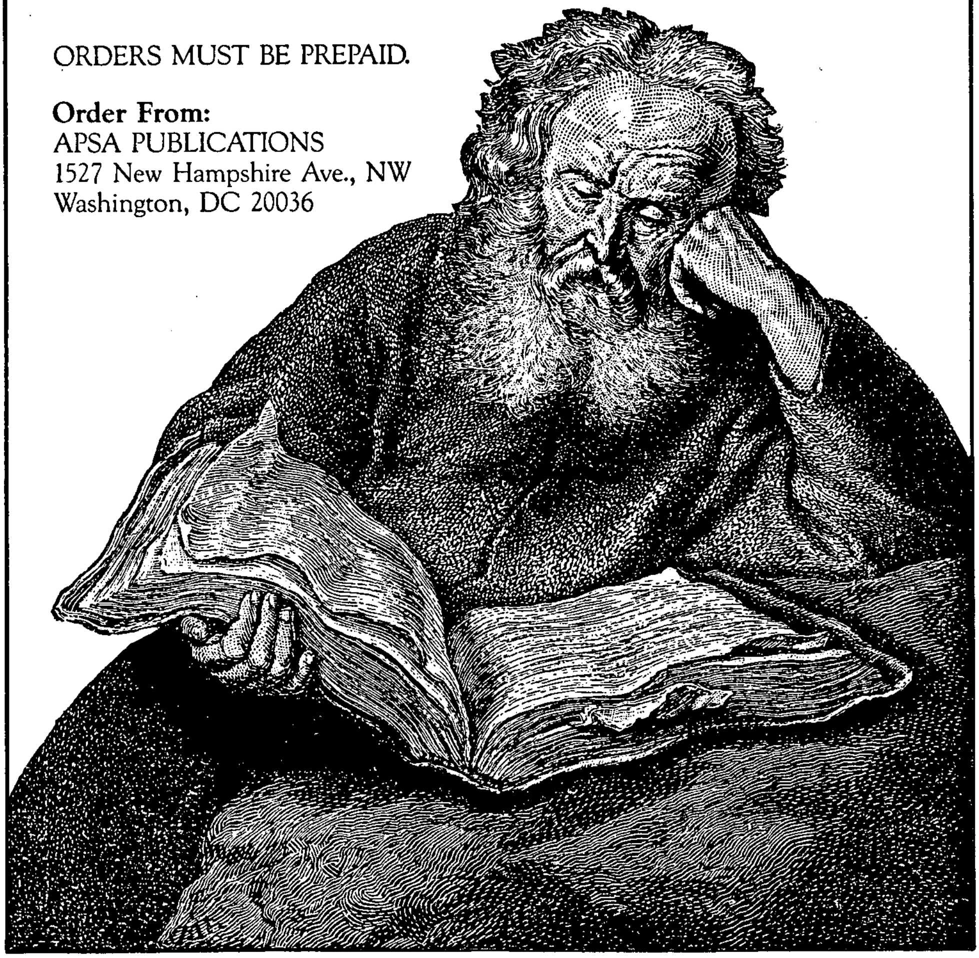

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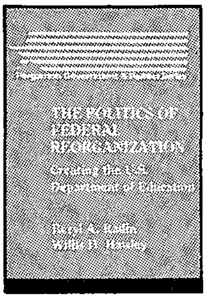
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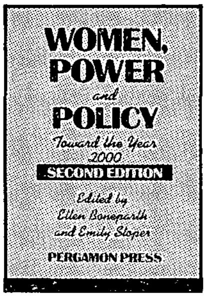

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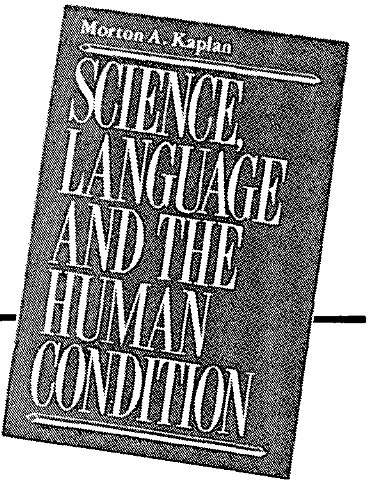

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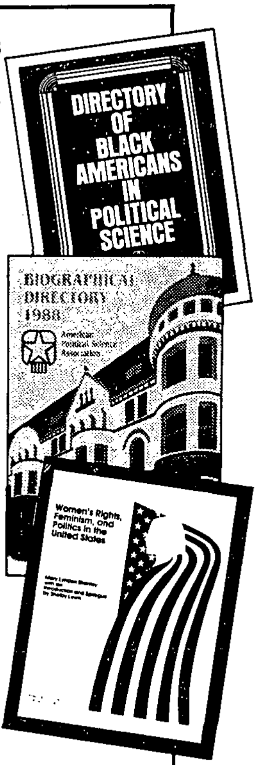
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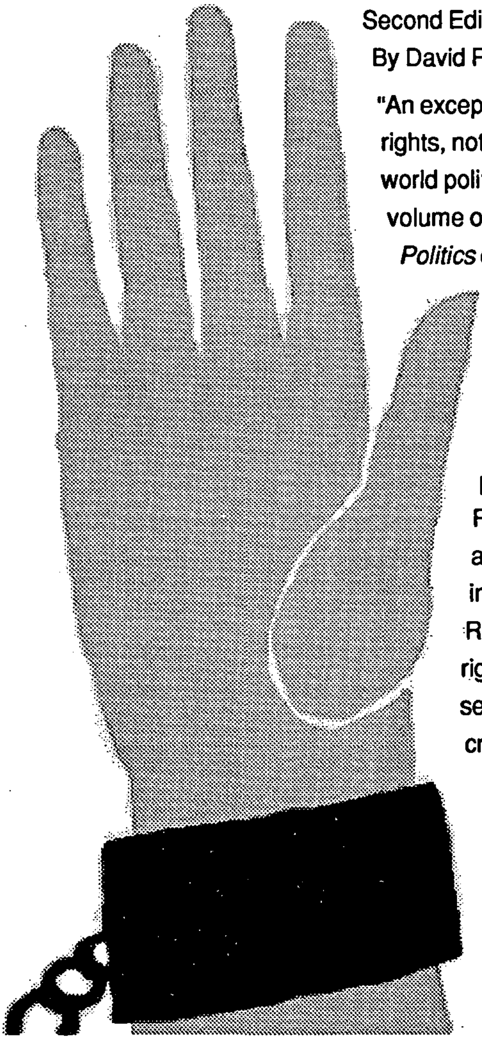

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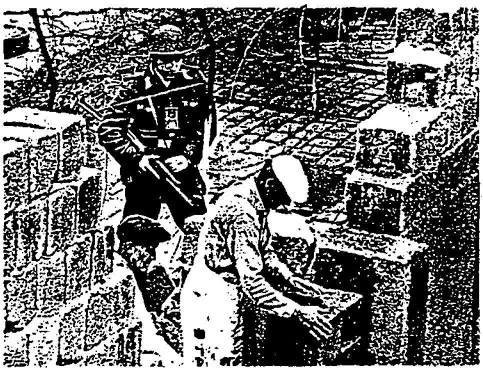

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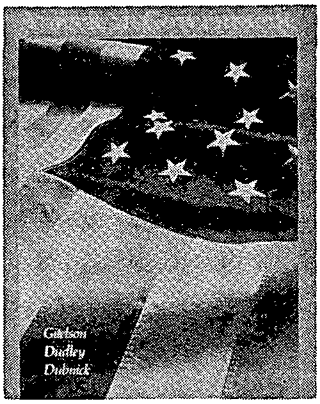

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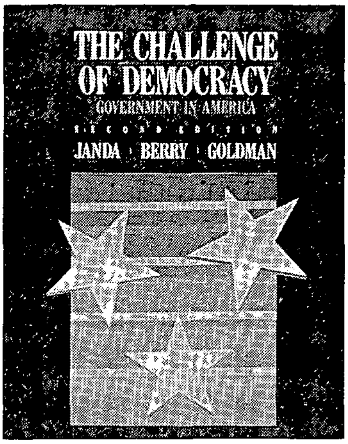

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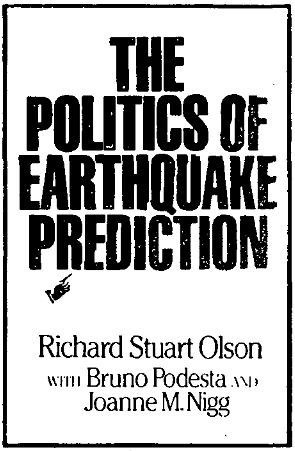

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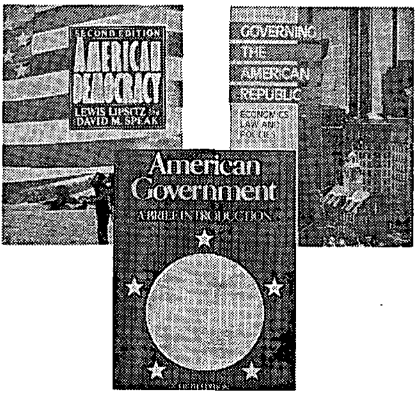

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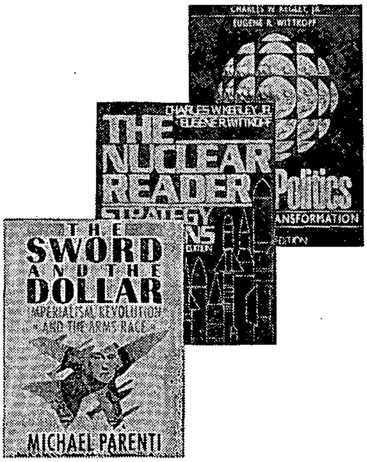

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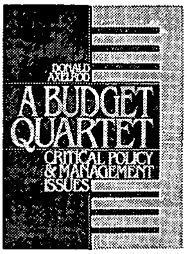

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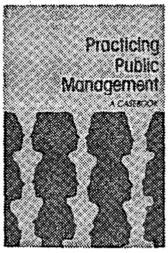

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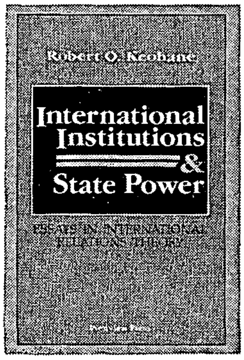

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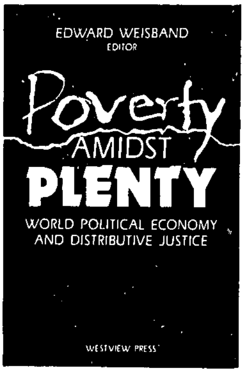

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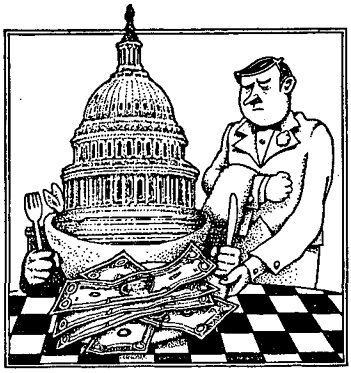

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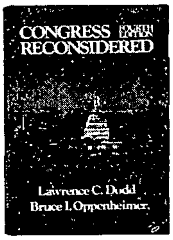
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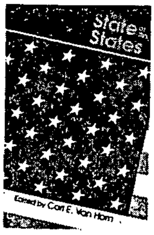
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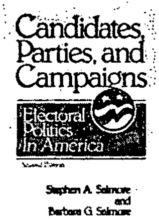
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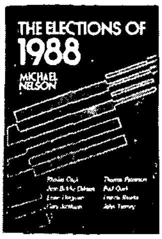

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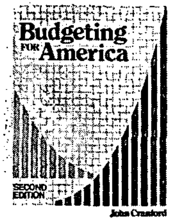

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